MU120R

The Open University

Mathematics and Computing
A first level multidisciplinary course

Open Mathematics

Readings

Contents

Readings for Unit 1

Cabbages are not spheres 4

School years 5

Reading for Unit 2

What price yesterday's pound in our pocket? 6

Reading for Unit 3

No let-up in the growth of low pay 10

Readings for Unit 4

Florence Nightingale 13

Chance encounters 21

Extract from *The Roots of Coincidence* 23

Reading for Unit 5

Guillemot 26

Readings for Unit 6

Blindly into the ditch 29

Mapping the AIDS pandemic 30

Images of the world 36

Reading for Unit 7

Narrative graphics of space and time 37

Reading for Unit 9

The Pythagorean Plato 38

Readings for Unit 10

The ultimate mile 40

Will women soon outrun men? 41

Reading for Unit 12

Logarithms 42

Readings for Unit 13

Sorry, no King Kongs 50

Motion control of MSVF lift drive 55

Bluebird correspondence 57

Readings for Unit 14

Extract from The Musgrave Ritual 62

Vermeer in perspective 64

The photographic accuracy of Vermeer's paintings 68

Cabbages are not spheres

Memory from Scotland. John Clearwater in the tiny kitchen preparing a salad of winter vegetables. He has a whole red cabbage in his hand that he is about to chop. Hope sees him staring at it. He holds it up to the light and then turns his head in her direction. He tosses the cabbage to her, which she catches. It is cool beneath her palms and surprisingly heavy. She chucks it back.

'Cabbages are not spheres,' he says.

'If you say so.' She smiles but she doesn't really know how to respond. This is the kind of remark he makes from time to time, cryptic, askew.

'Well, sort of spherical,' she says tentatively.

He cuts the cabbage in half and shows her the crisp violet and white striations, whorled like a giant fingerprint. The point of his knife traces the wobbling parabola of a leaf edge.

'These are not semi-circles.'

Hope sees what he is aiming at. 'A fir tree,' she ventures, 'is not a cone.'

John chops up the cabbage, swiftly and efficiently, like a chef, smiling to himself.

'Rivers do not flow in straight lines,' he says.

'Mountains are not triangles.'

'A tree ... a tree does not branch exponentially.'

'I give up,' she says. 'I don't like this game.'

Later, after their meal, he returns to the subject and asks her how she would set about measuring, precisely, the circumference of a cabbage. With a tape measure, Hope says.

'Every little bump and weal? Every bit of leaf-buckle?'

'Christ ... Take lots of measurements, get an average.'

'No precision, though. It's not going to work.'

He leaves the table and starts to jot ideas down in a notebook.

Hope now knows that this set him off down another path. He became preoccupied with the conviction that the abstract precision of geometry and measurement really had nothing to do with the imprecise and changing dimensions of living things, could not cope accurately with the intrinsic raggedness of the natural world. The natural world is full of irregularity and random alteration, but in the antiseptic, dust-free, shadowless, brightly lit, abstract realm of the mathematicians they like their cabbages spherical, please. No bumps, no folds, no dents or dinges. No surprises.

Source: William Boyd (1988) *Brazzaville Beach*

School years

School came to bore me. It took up far too much time which I would rather have spent drawing battles and playing with fire. Divinity classes were unspeakably dull, and I felt a downright fear of the mathematics class. The teacher pretended that algebra was a perfectly natural affair, to be taken for granted, whereas I didn't even know what numbers really were. They were not flowers, not animals, not fossils; they were nothing that could be imagined, mere quantities that resulted from counting. To my confusion these quantities were now represented by letters, which signified sounds, so that it became possible to hear them, so to speak. Oddly enough, my classmates could handle these things and found them self-evident. No one could tell me what numbers were, and I was unable even to formulate the question. To my horror I found that no one understood my difficulty. The teacher, I must admit, went to great lengths to explain to me the purpose of this curious operation of translating understandable quantities into sounds. I finally grasped that what was aimed at was a kind of system of abbreviation, with the help of which many quantities could be put in a short formula. But this did not interest me in the least. I though the whole business was entirely arbitrary. Why should numbers be expressed by sounds? One might just as well express a by apple tree, b by box, and x by a question mark. a, b, c, x, y, z were not concrete and did not explain to me anything about the essence of numbers, any more than an apple tree did. But the thing that exasperated me most of all was the proposition: If $a = b$ and $b = c$, then $a = c$, even though by definition a meant something other than b, and being different, could therefore not be equated with b, let alone with c. Whenever it was a question of an equivalence, then it was said that $a = a$, $b = b$, and so on. This I could accept, whereas $a = b$ seemed to me a downright lie or a fraud. I was equally enraged when the teacher stated in the teeth of his own definition of parallel lines that they met at infinity. This seemed to me no better than a stupid trick to catch peasants with, and I could not and would not have anything to do with it. My intellectual morality fought against these whimsical inconsistencies, which have forever barred me from understanding mathematics. Right into old age I have had the incorrigible feeling that if, like my schoolmates, I could have accepted without a struggle the proposition that $a = b$, or that sun = moon, dog = cat, then mathematics might have fooled me endlessly—just *how* much I only began to realize at the age of eighty-four. All my life it remained a puzzle to me why it was that I never managed to get my bearings in mathematics when there was no doubt whatever that I could calculate properly. Least of all did I understand my own *moral* doubts concerning mathematics.

Equations I could comprehend only by inserting specific numerical values in place of the letters and verifying the meaning of the operation by actual calculation. As we went on in mathematics, I was able to get along, more or less, by copying out algebraic formulas whose meaning I did not understand and by memorizing where a particular combination of numbers, for from time to time the teacher would say, 'Here we put the expression so-and-so', and then he would scribble a few letters on the blackboard. I had no idea where he got them and why he did it—the only reason I could see was that it enabled him to bring the procedure to what he felt was a satisfactory conclusion. I was so intimidated by my incomprehension that I did not dare to ask any questions.

Source: Carl Gustav Jung (1963) *Memories, Dreams, Reflections*

What price yesterday's pound in our pocket?

In the year 1300 a chicken cost a penny. Six centuries later, a nine-course dinner for two at the Carlton Hotel in London, including oysters, truffled soup, suprême de volaille, ortolans, champagne and liquers, cost £2 19s 6d. Were such prices 'a lot of money in those days'? Judge for yourself. The medieval penny chicken would have cost £1.84 at today's prices; the Edwardian binge about £170.

How do I know? Because I spent an hour at the Government's Central Statistical Office with Andrew Machin, head of operations of the Retail Price Index. A visit was necessary because no one publishes a year-by-year table of figures updating old prices—a statistical tool long sought by collectors, historians, chicken-fanciers and gourmets.

The familiar Retail Prices Index (RPI) is not a lot of help because it gives the inverse of the figures we want, charting the rise in prices (inflation) instead of the fall in value (purchasing power) of the pound. Also, as an index, it shows values relative to the base year 1987 (nominal value 100) and not 1994. (January 1994 is 141.3.)

However, with the help of a pocket calculator and a mathematical formula, it is possible to convert the base from 1987 to 1994 so that the RPI for past years is expressed in terms of its present-day purchasing power. A 1984 pound, for example, is equivalent to £1.58 today. Multiplying a price by the 1994-base RPI for its year gives its 1994 value.

Example: it is said to have been possible before the Second World War to visit the music hall, buy a cigar and a meal afterwards and still have change from a shilling. The 1994 value of a 1938 pound, calculated as above, is £32. So the 1938 shilling (5p in decimal currency) also rises 32 times, to become £1.60 at today's value. Cheap indeed—if the story is to be believed. Even if the true cost of a night out in 1938 was closer to 10 shillings than one, the calculation still gives a price of only £16. A more believable story is that in 1914 a bottle of whisky cost 3s 6d. The 1994 multiplier for a 1914 pound is 50.2: 3s 6d multiplied by 50.2 becomes £8.78 at today's value.

From recent newspaper reports, quoting historic prices yet groping for an accurate, up-to-date equivalent, I gleaned the following: in the 1530s, the artist Hans Holbein the Younger earned the 'princely salary' of £7 10s 0d a quarter. That would be £2,500 today. Hardly princely. In 1826, Sir Walter Scott, caught up in his publisher's bankruptcy, owed £120,000, 'several million pounds in today's money'. A bit vague. Today's equivalent would be nearly £4.5m. In 1958, Anthony Eden was paid an advance of £100,000 for his memoirs, 'a large amount in 1958' (£1,144,558, to be precise). In 1961, Thalidomide victims received compensation of £60,000 each, 'about £500,000 today' (£653,000, actually).

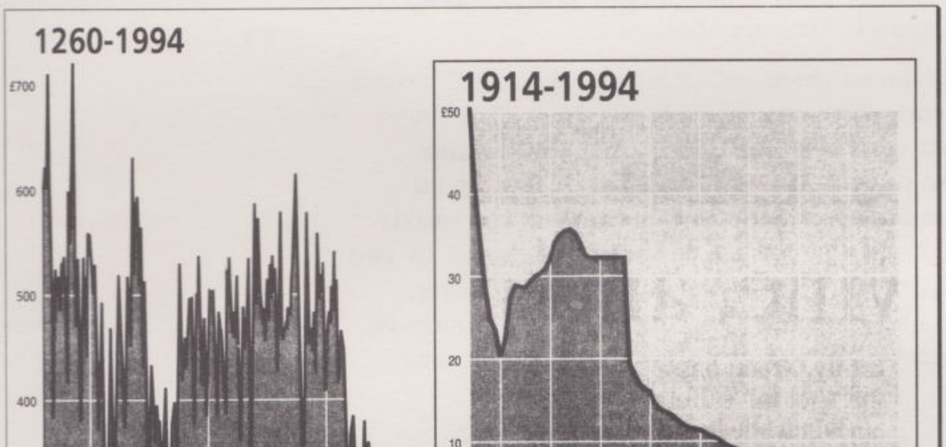
So far, so good. But Sir William Beveridge's cost-of-living index, precursor of the RPI, did not start until 1914. Where do the earlier statistics come from? Victorian and Edwardian students of the cost of living, such as Charles Booth and Seebohm Rowntree, for all their assiduousness, left us no more than statistical snapshots. Mr Machin and I were after a statistical video stretching back to feudal times.

We ended up splicing the three most reliable long-term statistical series we could find. Besides Beveridge (*Internal Purchasing Power of the Pound*, Table 10, CSO, 1994), Mr Machin chose the year-by-year statistical tables in a classic paper by Professor E. H. Phelps Brown and Sheila Hopkins,

'Seven Centuries of the Prices of Consumables, compared with Builders' Wage Rates' (*Economica*, Vol 23, pp. 296–314, 1956), which collates victualling records of ancient manors, monasteries, Cambridge colleges and the Royal Navy. The third was the Central Statistical Office's own 'unofficial' Prices Index for 1750–1914, a blend of Phelps Brown's prices and others published by a Cambridge historian, Walter Layton, in 1920.

The result is the graph of the value of the pound from 1260, with 1994 as base (equals 100p or £1). To use it, note the date of the price you wish to convert. Read off from the graph the 1994 value of the pound for that year. Then multiply the price and the 1994 pound value for the year to find its 1994 equivalent.

Here are some personal favourites. When launched in 1840 the penny post cost the equivalent of 16p today. In those days, the 'comfort-line' wage was reckoned to be £1 a week, equivalent to £38.44 today. Pub beer cost 2d a pint—32p in today's money, including duty of less than a tenth of today's 1p. The cheapest pint I ever bought, in 1958 at the Bird in Hand at Gosmore Hertfordshire, was a mild that cost me 1s 1d. A usurous 6.64d of that (51 per cent) was duty but the updated price is still only 62p. Today's pint—at £1.50 or so—carries a proportionately smaller duty burden, 24p, plus VAT at 17.5 per cent. Brewers kindly note.



The value of £1 at 1994 prices

Those who ate truffles at the Carlton in 1900 were toffs. But who, in 1300, would have bought the penny chicken? Not a farm labourer—even at a tempting present-day £1.84. Agricultural wages were 1d a day, 2d at the

most. Only medieval city toffs—merchants, craftsmen—could afford chickens for the pot. A serf might never buy one to eat. He kept and killed his own. Until the first Tudor monarch in 1485, he had little use for money. He grew his own vegetables on his lord's land in return for compulsory labour on certain days (and often compulsory chickens). His family built their cottage, spun, wove, baked, brewed and bartered.

It makes one wonder if cost-of-living figures can ever mean anything unless incomes are worked into them. After all, the gift of a pound would have meant more to a 15th-century labourer earning £5 a year (albeit equivalent, by then, to £2,500) than to a colossal earner such as Edward, Richard III's son, with £8,000–£10,000 a year (£4m–£5m today). These days, the same pound would be more valued by an occupant of a cardboard city than by the Duke of Westminster. Through whose eyes should we evaluate it?

The objective valuation device favoured by scholars of the cost of living is the 'basket' of consumables. Instead of calculating an average income which, in inequitable times, might be relevant to very few, statisticians take as their metaphorical constant the human stomach. Assuming that a tenfold increase in annual income will not result in a tenfold increase in the number of hot dinners consumed, they find out what the average person actually puts in his or her shopping basket, make it the base of a price index, then monitor price changes in the same basket.

Phelps Brown and Hopkins reviewed three sets of 'shopping basket' records dating from the 15th to the early 20th centuries. In all three, they found that food accounted for 80 per cent of expenditure. (Fuel and textiles made up the rest.) Their base period, 1451–75, was illuminated by an account book kept by William Savernak in 1453–60 recording the weekly 40 pence (equal to a building craftsman's wage of about £83) spent by two priests and a servant at Bridport, Dorset, on bread, meat, fish, butter, cheese, malt, hops, sugar, tea and fuel. Until recently, household budgets, like Savernak's, did not stretch beyond subsistence products. Shopping baskets do reflect income.

In 1300 serfs might not have bought chickens—but a medieval RPI shopping basket would not have contained any. Similarly, today's RPI contains no ocean-going yachts. Even a tiny fraction of one, included in a 'representative' basket, would be a distortion because the average household would never buy one.

The first index, in 1914, set up to protect workers from the 'temporary' economic consequences of the war, was a stark cost-of-living index, its 'indicators' mostly food. Alcohol was excluded but tobacco was assigned a grudingly small weighting (share) of the total spend. The basket included such staples as candles, mangles, back-lacing corsets, tram fares and shirt collars.

Today, only 14.4 per cent of the £277.63 weekly basket is devoted to food, so that the statistical boon of the unvarying basket of subsistence products has been lost. Sampling skills must make up for it. The RPI now spans 600 items bought by 7,000 households. The purchases of both pensioner households and the richest 4 per cent are excluded, to make the index more representative of the average household. Among the masses in the middle are hard-up people who value a pound more than others but whose purchasing power (or lack of it) still has a statistically significant effect on the price index.

A change from basic cost-of-living to RPI—a more accurate reflection of an increasingly affluent society—occurred in 1947 when alcohol was admitted to the basket, along with such beyond-subsistence luxuries as books, bicycles, radios and gramophone records. It was the first index to include fresh fruit. Clearly, the basket's contents were changing. But candles were not jettisoned until 1956, along with rabbits, mangles and distemper. In the same year, the RPI introduced coffee, brown bread, rice, pet food, televisions, washing machines and secondhand cars. Video and CD players arrived in 1987. Last year computer games came, saunas and jam tarts went. Foreign holidays were not included by the RPI's advisory committee until last year because of difficulties in calculating seasonal variation. As for chickens: roasting chickens replaced boiling fowl in 1962.

The price changes which have buffeted the basket over seven centuries have been caused by combinations of changes in the supply of consumables, the supply of precious metals used as standards of value, the supply of money (medieval debasement of the coinage, modern monetary meddling), and the size of the population. The Black Death of 1348–49 reduced the population by about a third, forcing wages up, provoking the first wage freeze (Statute of Labourers, 1351, as useless as its successors), the Peasants' Revolt of 1381, and six centuries of political conflict over the cost of living.

Swings and roundabouts have produced astonishing anomalies, such as constant prices between 1790 and the First World War. There were golden ages for consumers: the 15th century, when wages were high and food cheap, and the first half of the 18th century, when industrialization began to increase the supply and lower the price of manufactured goods, and meat dropped from 5d (£1.73) to 2d (70p) per lb. Living was cheap in the 1880s, too: income tax below a shilling in the pound, champagne 6s 6d a bottle (£18), oysters a shilling (£2.79) a dozen.

Hard times came during the 16th and 17th centuries, when food prices rose sevenfold, and the late 18th century, when prices rose 50–100 per cent. But today, 'Tudor inflation', a history-book horror story, seems tame compared with 1975's record annual rate of 24.2 per cent. It goes to show that high-taxing governments—the present government spends 45 per cent of the national income—are capable of wreaking greater havoc with the cost of living than belligerent agricultural workers short of the price of a chicken.

Source: *The Independent*, 26 February 1994

TABLE 1: NUMBER OF FULL-TIME EMPLOYEES WITH GROSS WEEKLY EARNINGS BELOW THE COUNCIL OF EUROPE'S DECENCY THRESHOLD* 1979-93

	1979		1982		1988		1991		1992		1993	
	mill	%	mill	%	mill	%	mill	%	mill	%	mill	%
Women	3.00	57.6	2.75	55.6	2.91	55.0	2.92	51.6	2.80	50.7	2.72	50.5
Men	1.64	14.6	1.83	17.7	2.77	26.7	2.81	27.6	2.97	28.7	2.73	29.3
All	4.64	28.3	4.58	30.0	5.68	36.2	5.72	36.1	5.77	36.4	5.45	37.0

Note: The figures for 1979 and 1982 are for men aged over 21 and for women aged over 18; the figures for 1988, 1991, 1992 and 1993 include all workers "on adult rates". Overtime earnings are excluded.
*£215.50 a week, £5.75 an hour in 1993/4.
Source: Low Pay Unit estimates based on *New Earnings Survey* 1993.

New government figures show that the proportion of the workforce earning below the Council of Europe's decency threshold has risen once again, continuing the trend since 1979.

The latest *New Earnings Survey*, published in October, shows less people in full-time work in 1993 As a consequence, the actual numbers of employees earning low pay has fallen. But of those in work, proportionately more are on low wages (Table 1).

The erosion of employment rights and job security, a severe weakening of union rights and high rates of unemployment combine to exert their influence on rates paid to some of the most vulnerable members of the British workforce: manual and public sector employees, the young, ethnic minority groups and women.

Whilst the proportion of working people suffering low pay is growing, so too is the trend towards inequality. The increasing gap between the

•Whilst the proportion of working people suffering low pay is growing, so too is the trend towards inequality.•

TABLE 2: PAY OF LOWEST DECILE EXPRESSED AS % OF HIGHEST DECILE

1978/9	41.3%
1990/1	30.5%
1991/2	30.2%
1992/3	30.0%

Source: *New Earnings Surveys*.

TABLE 3: EARNINGS OF MEN WORKING FULL-TIME IN MANUAL JOBS, 1886-1993

Year	Lowest decile as % of median	Median (£)	Highest decile as % of median
1886	68.6	1.21	143.1
1906	66.5	1.47	156.8
1938	67.7	3.40	139.9
1960	70.6	14.17	145.2
1970	67.3	25.60	147.5
1976	70.2	62.10	144.9
1979	68.3	88.20	148.5
1982	68.3	125.20	152.6
1986	65.4	165.50	154.8
1988	64.3	190.40	156.5
1990	63.7	224.20	157.9
1991	64.2	236.60	158.4
1992	63.6	251.90	157.4
1993	63.5	257.58	157.6

Note: Figures for later years have been adjusted by the Low Pay Unit to take account of statistical changes in the *New Earnings Survey*.
Source: *British Labour Statistics Historical Abstract 1886-1968*, HMSO, 1971, updated by *New Earnings Surveys* 1970-1993.

TABLE 4: AVERAGE MEAN GROSS WEEKLY EARNINGS FOR ALL FULL-TIME WORKERS BY REGION, APRIL 1993

Region	Weekly earnings	% of GB ave ('92 in brackets)	Increase in pay 92-93
Greater London	408.3	128.7 (126.5)	5.9
Remainder SE	328.7	103.7 (103.6)	4.2
North west	298.8	94.3 (93.7)	4.6
South west	298.4	94.2 (92.9)	5.4
Scotland	296.8	94.1 (94.1)	3.5
E Anglia	292.2	92.2 (94.7)	1.3
W Midlands	291.9	92.1 (91.9)	4.3
North	288.6	91.1 (92.7)	2.2
Yorks & Humbs	287.4	90.7 (91.0)	3.7
E Midlands	285.7	90.2 (90.6)	3.5
Wales	281.2	88.7 (88.9)	3.8
GB	316.9	100.0	4.0

Source: *New Earnings Survey* 1993 Part A.

No let-up in growth of low pay

The LPU's analysis of the government's *New Earnings Survey 1993* gives no cause for cheer, with the proportion of employees on low pay continuing to grow. *The New Review* presents the facts.

TABLE 5: PROPORTION OF FULL-TIME ADULT EMPLOYEES WITH GROSS WEEKLY EARNINGS BELOW £215.50 A WEEK (DECENCY THRESHOLD)

Region	Sub-region	Below threshold
South west	Cornwall	50.1%
	Wilts	29.7%
South east	Isle of Wight	40.4%
	Berks	19.6%
Wales	Mid-Glamorgan	43.5%
	South Glamorgan	33.7%
Scotland	Borders	51.5%
	Grampian	31.4%

Source: *Hansard*, November 1993.

TABLE 6: AVERAGE GROSS WEEKLY EARNINGS INCLUDING THE EFFECTS OF OVERTIME, FULL-TIME EMPLOYEES AGED 18 AND OVER, 1979-1993

Year	Men £	Women £	Women's earnings as % of men's
1979	99.0	63.0	63.6
1981	137.0	91.4	66.7
1983	163.3	108.3	66.6
1985	190.4	125.5	65.9
1986	205.5	136.3	66.3
1987	222.1	147.2	66.2
1988	243.8	163.4	67.0
1989	266.9	181.3	67.9
1990	292.8	200.4	68.4
1991	316.2	221.7	70.1
1992	337.3	235.9	71.1
1993	350.5	250.9	71.6

Source: *New Earnings Survey* 1993 Part A.

TABLE 7: AVERAGE GROSS HOURLY EARNINGS EXCLUDING THE EFFECTS OF OVERTIME, FULL-TIME EMPLOYEES AGED 18 AND OVER, 1979-1993

Year	Men (pence)	Women (pence)	Women's earnings as % of men's
1979	226.9	165.7	73.0
1981	322.5	241.2	74.8
1983	387.6	287.5	74.2
1985	445.3	329.9	74.1
1986	481.8	358.2	74.3
1987	521.3	383.8	73.6
1988	568.3	426.8	75.1
1989	622.8	475.6	76.4
1990	682.3	525.0	76.9
1991	750.2	587.1	78.3
1992	803.0	635.0	79.1
1993	839.0	664.0	79.1

Source: *New Earnings Survey* 1993 Part A.

lowest and highest paid groups is seen in Table 2, which presents the lowest tenth of pay levels as a percentage of the highest tenth. Since 1979 there has been a substantial growth in pay inequality.

Table 3 shows that the gap between the highest and lowest earners is greater now than it was when records began in 1886.

Regional analysis also shows wide variations in pay levels. Table 4 shows average pay in Wales is just 88.7 per cent of average pay for Great Britain as a whole, and only 68.9 per cent that of Greater London. Pay increases show similar variations.

Likewise within regions pay figures show significant differences, as Table 5 indicates.

'Average pay in Wales is just 88.7 per cent of average pay for Great Britain as a whole, and only 68.9 per cent that of Greater London.'

TABLE 8: YOUNG PEOPLE'S GROSS WEEKLY EARNINGS AS A PERCENTAGE OF ALL OVER 21s

Year	<18	18-20
1979	42.3	60.8
1986	38.2	54.6
1988	39.4	53.9
1989	38.8	53.8
1990	38.3	53.5
1991	37.4	53.3
1992	36.1	51.6
1993	33.9	49.9

Source: *New Earnings Survey* 1993 Part A.

The gap between men's and women's pay remains largely untouched. Table 6 shows women's progress towards equality virtually halted last year. In fact, the figures excluding overtime (Table 7) show that progress came to a complete standstill between 1992 and 1993. This marks the end of (albeit painfully slow) progress towards equality which has been in train over the last few years.

Occupational segregation is perhaps an even greater problem for women. Very few break through the middle ranks of hierarchies to reach the upper echelons, even where personnel policies are designed to overcome obstacles.

In occupations and professions dominated by women, recent reports indicate that top managerial posts are still almost exclusively filled by males.

Although there is evidence that some women in managerial grades have been making ground on their male counterparts, equality is nowhere in sight. As the International Labour Office calculated, at currrent rates of progress it will take 475 years to achieve equality!

Recent evidence suggests that the recession and growing male unemployment may be responsible for some organisations going back on their commitment to equality. Women's pay makes better progress when the labour market is buoyant. Under the circumstances the prognosis is bleak.

'The plight of young people in the labour market refutes the government's suggestion that the route to employment creation is in lowering wage levels.'

Starkest evidence of all pay disparities can be seen in the figures which compare young people's rates with those of adults (Table 8). The 18 to 20 age group have been particularly hard hit. Their pay compared to the adult (over 21s) average has slumped from 60.8 in 1979 to 49.9 per cent in 1993, a drop of 11.9 percentage points. In today's money this represents a fall of £34.54 a week.

The under 18s have also suffered; their pay compared to the adult average has fallen 8.4 percentage points since 1979, with a particularly sharp drop of 2.2 percentage points this year alone. For doing a full-time job, they now average barely one-third of an adult's wage.

The LPU's Employment Rights Advice Service has evidence of pay levels that the average conceals. During 1993 the LPU rights advisers took hundreds of enquiries from young people. Examples include a 19 year old office junior doing clerical work earning £57.69 for a 35 hour week, an 18 year old hairdresser on £35 for a 40 hour week and a 19 year old trainee motor mechanic getting £1 an hour for a 35 hour week.

The plight of young people in the labour market refutes the government's suggestion that the route to employment creation is in lowering wage levels. Despite this evidence of continuing deterioration in pay and already high levels of unemployment, the young account for the fastest growing group amongst the unemployed.

Source: *The New Review*, December 93/January 94

Florence Nightingale

She saved the lives of thousands of soldiers in the Crimea and was one of the founders of modern medical care. She was also a pioneer in the uses of social statistics and in their graphical representation

by I. Bernard Cohen

Florence Nightingale is remembered as a pioneer of nursing and a reformer of hospitals. She herself saw her mission in larger terms: to serve humanity through the prevention of needless illness and death. For much of her long life (1820–1910) she pursued this mission with a fierce determination that gave everything she did a singular coherence. Her greatest contributions were undoubtedly her efforts to reform the British military health-care system and her establishment, through the founding of training programs and the definition of sound professional standards, of nursing as a respected profession. Much of what now seems basic in modern health care can be traced to pitched battles fought by Nightingale in the 19th century. Less well known, because it has been neglected by her biographers, is her equally pioneering use of the new advanced techniques of statistical analysis in those battles.

Nightingale learned at first hand as chief nurse during the Crimean War (1854–56) that improved sanitary conditions in military hospitals and barracks could sharply cut the death rate and save thousands of lives. Her battle was to convince skeptical men in power. At a time when the collection and analysis of social statistics was still uncommon Nightingale recognized that reliable data on the incidence of preventable deaths in the military made compelling arguments for reform. Thus in addition to advancing the cause of medical reform itself she helped to pioneer the revolutionary notion that social phenomena could be objectively measured and subjected to mathematical analysis.

Nightingale's achievements are all the more impressive when they are gauged against the background of social restraints on women in Victorian England. Her father, William Edward Nightingale, was an extremely wealthy landowner, and the family moved in the highest circles of English society. In those days women of Nightingale's class did not attend universities and did not pursue professional careers; their purpose in life was to marry and bear children. Nightingale was fortunate: her father believed women should be educated, and he personally taught her Italian, Latin, Greek, philosophy, history and—most unusual of all for women of the time—writing and mathematics. When in her early twenties Nightingale expressed an interest in nursing, her father took that interest seriously enough to consult with physicians about the suitability of such a career.

If pursuing any career was a radical step for a woman of Nightingale's social class, however, taking up nursing seemed out of the question even in her enlightened family. It was not "the physically revolting part of a hospital" that offended William Nightingale so much as what seemed to be overwhelming evidence of the dissolute habits of nurses. Nurses in those days lacked training; they were almost always coarse and ignorant women, given to promiscuity and drunkenness. Nightingale herself later told her father she had been informed by the head nurse in a London hospital that she "had never known a nurse who was not drunken" and that most of the nurses engaged in "immoral conduct" with the patients in the wards. Not surprisingly, her parents hoped their daughter would give up her unusual ambition, marry and settle down.

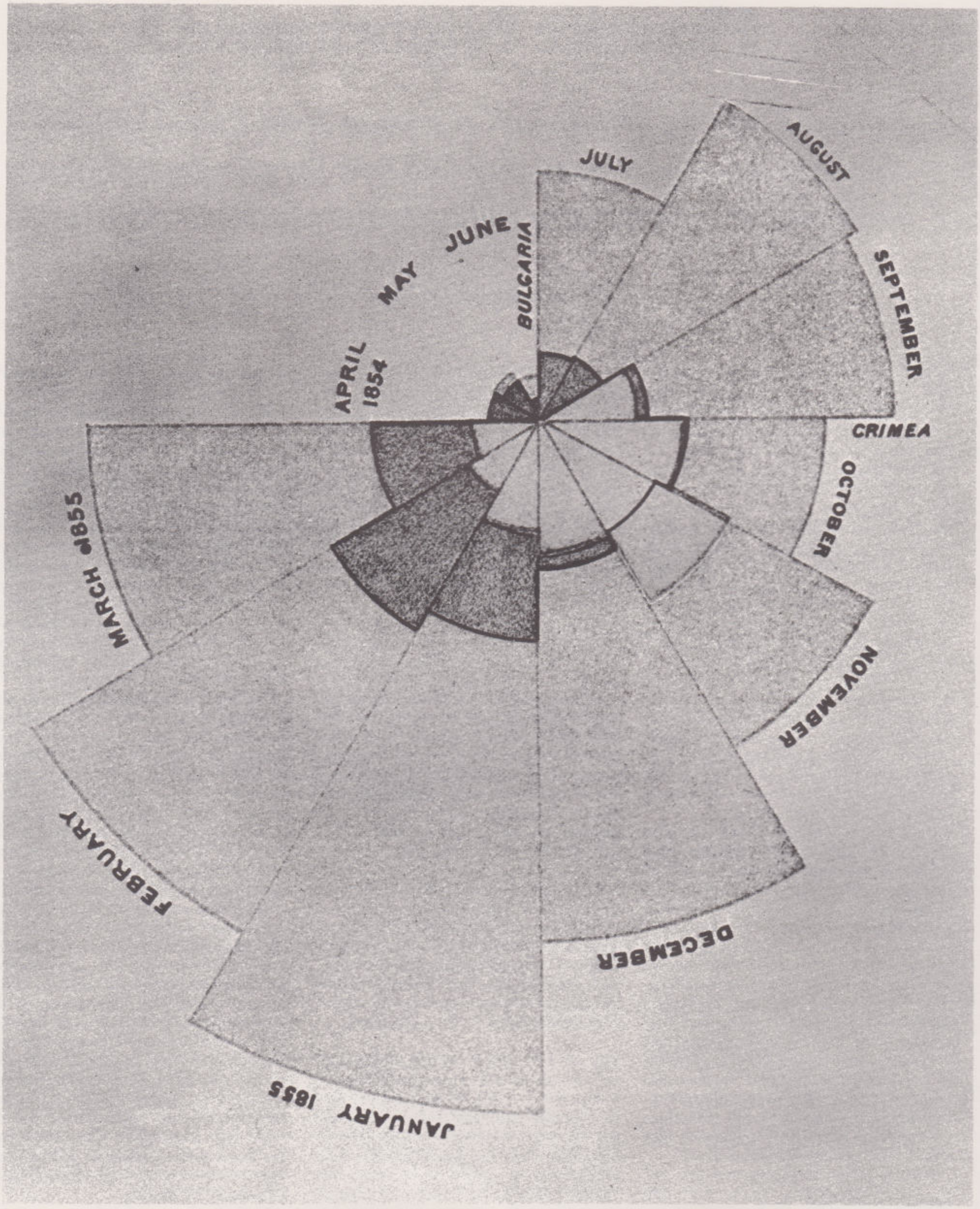
By all accounts Florence Nightingale was an attractive young woman, and it was not for lack of opportunity that she rejected marriage. Indeed, she once was tempted to accept a suitor, but after a long courtship she reluctantly concluded that she could never satisfy her "moral" and "active" nature "by spending a life with him in making society and arranging domestic things." Conventional marriage, she wrote in her diary, meant "to be nailed to a continuation and exaggeration" of her "present life," a prospect that seemed to her "like suicide." God, she decided, had envisioned for her a different fate. She was one of those whom he "had clearly marked out... to be single women."

When her parents forbade her to take up nursing, Nightingale turned for comfort to religion. It was to remain a driving force in her life. Her religious feelings, however, centered on the conviction that the best way to serve God was through service to mankind. Thus in the difficult years of her twenties she did not give up her ambition to pursue a career; she read voraciously on medicine and health care, spent some time inspecting hospitals in London and worked privately with children of the slums, whom she called her "little thieves at Westminster." Still, she was frustrated.

Finally in 1851 Nightingale was able to break away from home, spending three months near Düsseldorf in Germany at a hospital and orphanage run by a Protestant order of "deaconesses." Later, in spite of the protests of her family, she served an apprenticeship at another hospital, this one operated by the Sisters of Mercy in St. Germain, near Paris. At the age of 33 she was at last starting out in her chosen profession.

Returning to London in 1853, Nightingale soon got her first "situation" (an unpaid one) as superintendent of a London "establishment for gentlewomen during illness." Her job was to supervise the nurses and the functioning of the physical plant and to guarantee the purity of the medicines. Although she succeeded in creating a model institution by the standards of the day, one that was open to patients of all classes and religions, she was disappointed that she could not accomplish what even then she had come to consider her primary aim: the establishment of a formal training school for nurses.

Nightingale stayed only a year at her first job, because greater opportunities awaited her. In September, 1854, British and French troops invaded the Crimea, on the north coast of the Black Sea, in support of Turkey in its dispute with Russia. (Russia had long had territorial ambitions in Turkey, particularly with



POLAR-AREA DIAGRAM was invented by Florence Nightingale to dramatize the extent of needless deaths in British military hospitals during the Crimean War (1854–56). She called such diagrams "coxcombs." The area of each colored wedge, measured from the center, is proportional to the statistic being represented. Blue wedges represent deaths from "preventable or mitigable zymotic" diseases (contagious diseases such as cholera and typhus), pink wedges deaths from wounds and gray wedges deaths from all other causes. Mortality in the British hospitals peaked in January, 1855, when 2,761 soldiers died of contagious diseases, 83 of wounds and 324 of other causes, for a total of 3,168. Based on the army's average strength of 32,393 in the Crimea that month, Nightingale computed an annual mortality rate of 1,174 per 1,000. The diagram is taken from Nightingale's book *Notes on Matters Affecting the Health, Efficiency and Hospital Administration of the British Army* (1858); half of the diagram, representing the period from April, 1855, to March, 1856, does not appear.

regard to Constantinople, the Orthodox holy city; one of the proximate causes of the Crimean War was the Russian demand that it be given a protectorate over the Orthodox subjects of the Turkish sultan.) The allied forces scored a quick victory at the Battle of the Alma River on September 20, and then began a siege of the Russian naval base at Sevastopol. Public jubilation in Britain soon turned to dismay when the Crimean correspondent of *The Times,* William Howard Russell, reported that sick and wounded British soldiers were being left to die without medical attention. Not only were there too few surgeons and "not even linen to make bandages" but also there was not a single qualified nurse in the British military hospital at Scutari (near Constantinople). The French, on the other hand, had sent 50 Sisters of Mercy to the Crimea.

It was a golden opportunity for the ambitious Nightingale. She immediately wrote to a longtime friend, Sidney Herbert, the "Secretary at War," to volun-

FLORENCE NIGHTINGALE is pictured in a photograph taken in later life. Beginning soon after her return from the Crimea in 1856 until her death in 1910 at the age of 90, she lived as an invalid, largely confined to her bedroom. Her illness may not have been organic, but it did not prevent her from exercising her influence by receiving frequent visitors and by maintaining an extensive correspondence.

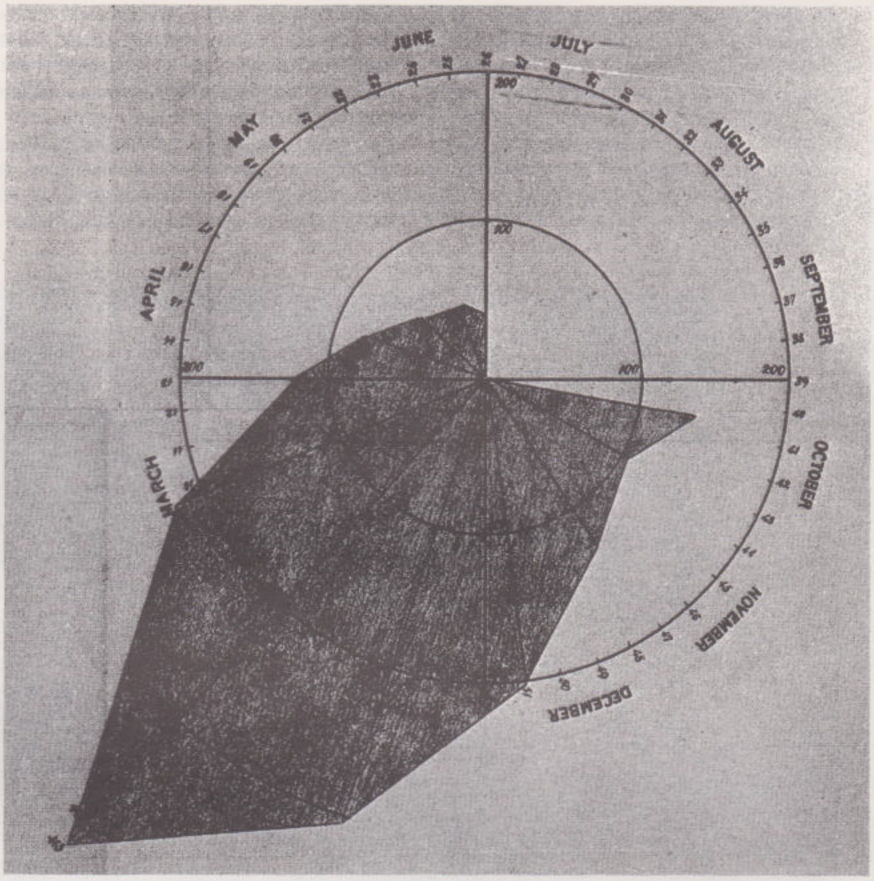
teer her services. As it happened, a letter from Herbert was already on its way to her, asking her to recruit a corps of trained nurses and lead them to Scutari. When Nightingale left for Turkey on October 21, 1854, accompanied by 38 nurses, she had the official backing of the government (although not of the army) and, perhaps more important, the private financial support of a special fund raised by *The Times*. Besides making her an international heroine, her work in the Crimea and the conditions she saw there were to determine her mission for the rest of her life.

The conditions Nightingale and her party found when they arrived at Scutari on November 5, the day of the major Battle of Inkerman, were appalling. The hospital barracks was infested with fleas and rats. Under the buildings, as a commission of inquiry later reported, "were sewers... loaded with filth... through which the wind blew sewer air up the pipes of numerous open privies into the corridors and wards where the sick were lying" on straw mats, in a state of overcrowding that got even worse after Inkerman. The canvas sheets, according to Nightingale, were "so coarse that the wounded men begged to be left in their blankets"; moreover, the laundry was done in cold water, with the result that many linens returned as clean were so "verminous" that they had to be destroyed. Essential surgical and medical supplies were lacking, or their distribution was blocked by military red tape.

These were the conditions that awaited patients arriving at Scutari after a slow sea voyage across the Black Sea and through the Bosporus, weak and emaciated, suffering from frostbite and dysentery as well as from their wounds. In fact, the resulting epidemics of cholera and typhus, and not the injuries themselves, caused the greatest loss of life at Scutari. In February, 1855, the mortality rate at the hospital was 42.7 percent of the cases treated.

In her efforts to establish an effective hospital in Turkey, Nightingale showed real skill as an administrator. At every step, however, she was hampered by the military authorities, who resisted any change that might seem to be a concession of their own errors or incompetence. The military men resented the fact that Nightingale's authority was independent of the armed services, that she was a civilian and—far worse—that she was a woman. Hostility to her mission ran so high that at first her nurses were not allowed on the wards. Even after she had achieved greater acceptance she had to struggle against petty officials, such as a supply officer who refused to distribute badly needed shirts from his store until the entire shipment of 27,000 could be inspected by an official of the Board of Survey.

In the face of such impediments it



MORTALITY RATE at Scutari, the main British hospital in the Crimean War, declined sharply after sanitary improvements were made under Nightingale's influence. In the winter of 1854–55 the British army besieging the Russian fortress at Sevastopol was ravaged by malnutrition, exposure and disease: dysentery, cholera, typhus and scurvy. The death rate at Scutari, calculated here by Nightingale on an annual basis as a fraction of the patient population, reached 415 percent in February. Sanitary reforms began in March. This diagram is taken from the report of a Royal Commission set up after the war to investigate sanitary conditions in the army.

was Nightingale's independence from the military and, above all, her private source of funds that enabled her to accomplish so much at Scutari. She established her own laundry, including boilers to heat the water; she installed extra kitchens in the hospital; she became, finally, the supplier of the entire hospital, "a kind of General Dealer in socks, shirts, knives and forks, wooden spoons, tin baths, tables and forms, cabbage and carrots, operating tables, towels and soap, small tooth combs, precipitate for destroying lice, scissors, bed pans and stump pillows." The money for these supplies and for the staff she recruited came not only from *The Times* fund but also from other philanthropists and from her own private funds.

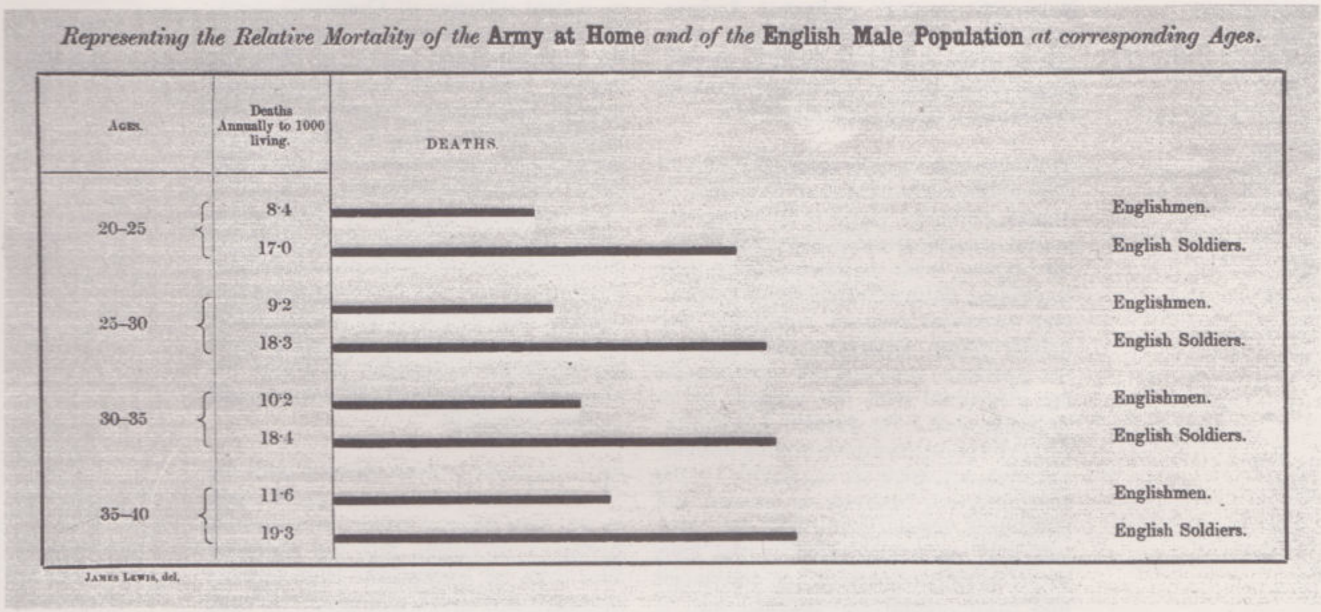
While Nightingale was carrying out her administrative duties she still found time to attend to the sick herself, late at night, on endless rounds that gave rise to the legend of the "ministering angel" of the Crimea. At night she banned all other women from the wards (she had been obliged to send some of her nurses home for delinquent behavior) and made her way, according to the commissioner of *The Times* fund, "alone, with a little lamp in her hand," through "those miles of prostrate sick." Longfellow immortalized this "lady with a lamp" image in his poem of 1857 ("Lo! in that house of misery / A lady with a lamp I see"). There is, however, a more significant measure of Nightingale's accomplishment, one that she herself stressed: by the spring of 1855, half a year after she arrived at Scutari, mortality in the hospital had dropped from 42.7 percent to 2.2 percent.

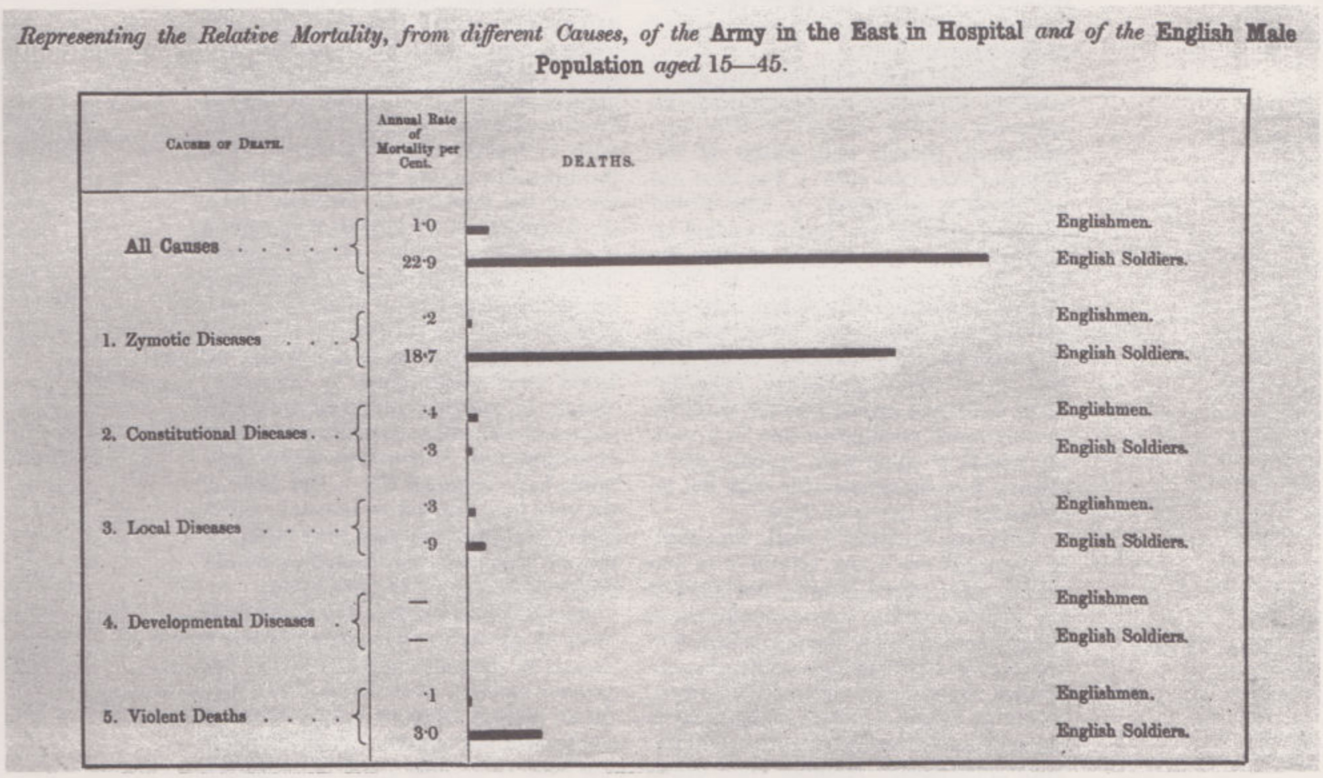
Nightingale returned to England in July, 1856, four months after the end of the war. By that time, at the age of 36, she was a world-famous and revered figure. She nonetheless shunned all attempts to honor her publicly, deciding instead that the most appropriate recognition for her services would be the establishment of a commission to investigate military medical care. In the Crimea, she wrote, some 9,000 soldiers were lying "in their forgotten graves," dead "from causes which might have been prevented." The tragedy of needless death was continuing in every army

barracks and hospital, even in peacetime. It could be ended only by instituting throughout the Army Medical Service the same sanitary reforms that had saved so many lives at Scutari. This was the task Nightingale set herself.

How could she convince people of the need for reform? Nightingale saw that the most compelling argument would be statistical. The idea of using statistics for such a purpose—to analyze social conditions and the effectiveness of public policy—is commonplace today, but at that time it was not. The science of social statistics was in its infancy, and in promoting the cause of medical reform Nightingale became a promoter of the new tool as well.

Seen simply as the collection of numerical data, statistics have a long history (going back at least to the Book of Numbers of the Old Testament), but the analysis of such data is only as old as the scientific revolution of the 17th century. Early attempts to analyze data on social phenomena were hampered by inadequacies both in the data themselves and in the mathematical tools of analysis. According to the historian of statistics Helen M. Walker, the rise of modern statistics in the 19th century had three roots: the development of the mathematical theory of probability, the emer-





LINE DIAGRAMS from the Royal Commission's report compare conditions in the army to those in civilian life. Mortality in the peacetime army in Britain was nearly twice as high as it was among civilians (*top*). In the Crimean War "zymotic" diseases were the main causes of death and were far more prevalent than they were in England (*bottom*). Figures in the top diagram are percentages; those in the bottom one are per 1,000. The report led to the adoption of a sanitary code for the army and to a series of physical improvements in military buildings. Like other diagrams in report, these are examples of Nightingale's innovative approach to representation of statistics.

gence of the modern state with its agencies for collecting information on its citizens and their activities, and the theoretical interest of political economists in finding causes for human social behaviors. These "three movements," Walker wrote, were pulled together in the career of the mid-19th-century Belgian astronomer-statistician Lambert-Adolphe-Jacques Quételet, widely regarded as the founder of modern social statistics. In 1841 Quételet organized Belgium's central statistical bureau, which became a model for similar agencies in other countries, and his international leadership in statistical research continued until his death in 1874.

Nineteenth-century scholars trying to make a science out of the study of human behavior faced a dilemma: the model science of those days was classical physics, with its deterministic laws describing natural phenomena, but human behavior seemed individual and indeterminate. Quételet's resolution of the problem bypassed the question of the individual with the concept of an "average man." He showed that whereas there are no laws determining individual behavior, there are regularities in the attributes and behavior of groups, and that these regularities could be characterized mathematically by the laws of probability. Quételet was convinced that even mental and moral traits, if only they could be measured accurately, would also follow regular laws of statistical distribution.

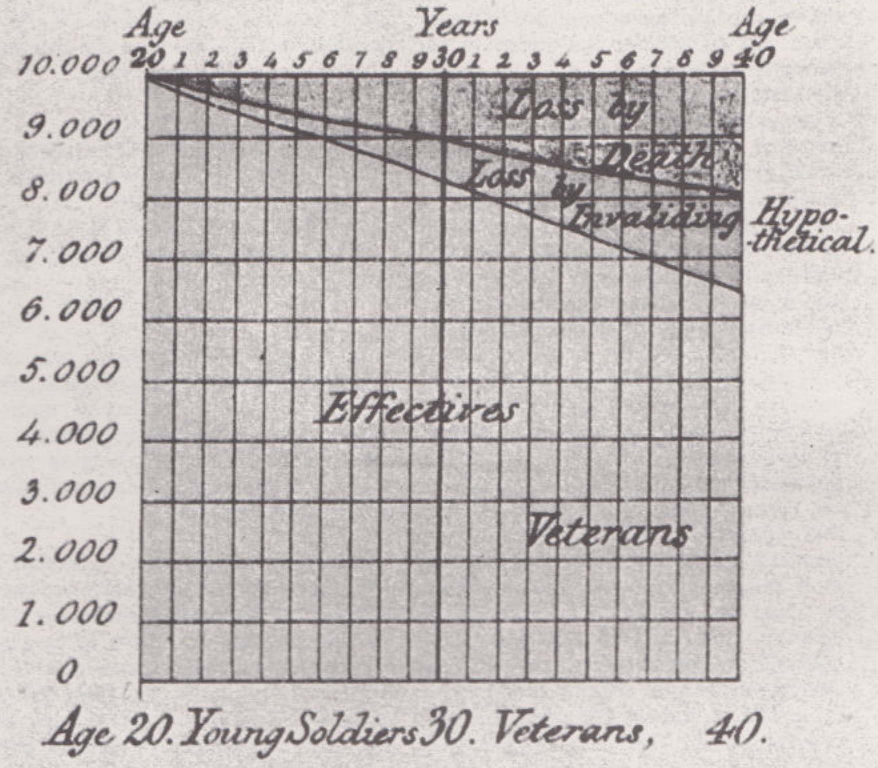
Quételet's most original and most startling work was his analysis of the influence of such factors as sex, age, education, climate and season on the French crime rate (1831). The data did not allow a prediction of who would commit what crime, but according to Quételet they did display regularities that would enable a scientist to "enumerate in advance how many individuals will stain their hands in the blood of their fellows, how many will be forgers, how many will be poisoners." The discovery of these regularities led Quételet to the radical conclusion that "it is society which, in some way, prepares these crimes, and the criminal is only the instrument that executes them."

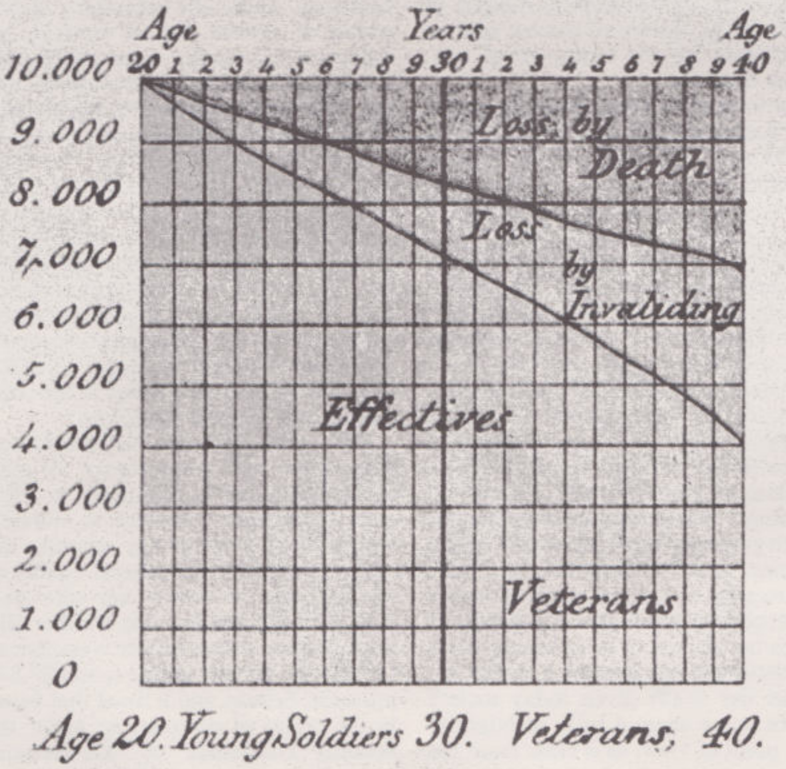
Although Quételet's work was highly regarded by many scholars, it was abhorred by others. The determinism of his "social physics" was an anathema to people committed to the prevailing doctrines of free will and individual responsibility. John Stuart Mill, for example, wrote at length against probability in general and its application to social science in particular. Another vocal opponent of the statistical view of man and society was Charles Dickens. His novel *Hard Times* was meant to satirize those people, Dickens later said, who could see nothing but "figures and averages," those "addled heads" who would use the yearly average temperature in the Crimea "as a reason for clothing a soldier in nankeens [silks] on a night when he would be frozen to death in fur." Dickens disliked the statistical view because he thought it was dehumanizing, and in *Hard Times* he portrayed the regularities found by statisticians in the rate of insanity, crime, suicide and prostitution as a "deadly statistical clock."

Nightingale, on the other hand, was an ardent admirer of Quételet's work, and she early displayed a predilection for collecting and analyzing data. At Scutari, apart from the all-important sanitary reforms she instituted, she also systematized the chaotic record-keeping practices; until then even the number of deaths was not known with accuracy. When she returned to England in 1856, she met William Farr, a physician and professional statistician. Under Farr's guidance Nightingale soon recognized the potential of the statistics she had gathered at Scutari, and of medical statistics in general, as a tool for improving medical care in military and civilian hospitals.

Throughout military history until the 20th century the main cause of death in war was disease rather than wounds sustained in battle, and the Crimean War was no exception. Nightingale's numbers still speak eloquently. During the first months of the Crimean campaign there was "a mortality among the troops at the rate of 60 percent per annum from *disease* alone," a rate exceeding that of the Great Plague of 1665 in London and higher also "than the mortality in cholera to the attacks" (that is, the mortality among those who had contracted the disease). In January, 1855, the mortality in all British hospitals in Turkey and the Crimea, measured in relation to the entire army in the Crimea but not including men killed in action, peaked at an annual rate of 1,174 per 1,000. Of this number 1,023 deaths per 1,000 were attributable to "zymotic" disease (a category introduced by Farr including epidemic, endemic and contagious disease). This means that if mortality had persisted for a full year at the rate that applied in January, and if the dead soldiers had not been replaced, disease alone would have wiped out the entire British army in the Crimea.

Nightingale's various methods of calculating mortality dramatized both the impact of disease and the effects of improved sanitary conditions. Calculated on an annual basis as a percentage of the patient population, the death rate at the Scutari hospital reached an incredible 415 percent in February, 1855. In March, however, Nightingale's sanitary reforms began to be implemented and mortality among the patients declined precipitously. By the end of the war,





according to Nightingale, the death rate among sick British soldiers in Turkey was "not much more" than it was among healthy soldiers in England; even more remarkable, the mortality among all British troops in the Crimea was "two-thirds only of what it [was] among our troops at home."

The comparison suggested that the soldiers at home were living in their barracks under unhealthy conditions. After Farr had made Nightingale aware of the significance of mortality tables, she at once thought of comparing the mortality among civilians to that among soldiers. She found that in peacetime soldiers in England between the ages of 20 and 35 had a mortality rate nearly twice that of civilians. It is just as criminal, she wrote in 1857, "to have a mortality of 17, 19, and 20 per thousand in the Line, Artillery and Guards in England, when that of Civil life is only 11 per 1,000, as it would be to take 1,100 men per annum out upon Salisbury Plain and shoot them." (The 1,100 represented 20 per 1,000 of an enlisted force of 55,000.) Clearly the need for sanitary improvements in the military was not limited to hospitals in the field. By pressing her case with these statistics Nightingale eventually gained the attention of Queen Victoria and Prince Albert, as well as of the prime minister, Lord Palmerston. In spite of the passive resistance of the War Office, Nightingale's wish for a formal investigation of military health care was granted in May, 1857, with the establishment of a Royal Commission on the Health of the Army.

It would not have been possible at that time for a woman to serve on such a board. Nightingale nonetheless strongly influenced the commission's work, both because some of its members were her friends (including Sidney Herbert, the minister who sent her to the Crimea) and because she provided it with much of its information. As a statement of her own views she wrote and had privately printed an 800-page book titled *Notes on Matters Affecting the Health, Efficiency and Hospital Administration of the British*

LOSS OF MANPOWER in the British army due to excess mortality and invaliding is illustrated by diagrams from the report of the Royal Commission. Both graphs assume that 10,000 20-year-old recruits are added to the force annually and that a healthy soldier's career lasts for 20 years. Each small rectangle represents 1,000 men. Under the existing unhealthy conditions (*bottom*) death (*brown*) and invaliding (*yellow*) reduce the strength of the army (*beige*) to 141,764 from its maximum size of 200,000, a loss of 29 percent. If mortality were as low as it was in the civilian population and the relation between mortality and the invaliding rate stayed the same, the report concluded, the strength of the army would increase significantly, to 166,910 (*top*).

Army, which included a section of statistics accompanied by diagrams. Farr called it "the best [thing] that ever was written" either on statistical "Diagrams or on the Army."

Nightingale was a true pioneer in the graphical representation of statistics: she invented polar-area charts, in which the statistic being represented is proportional to the area of a wedge in a circular diagram. Nightingale used these diagrams, which she called her "coxcombs" because of their vivid colors, to dramatize the extent to which deaths in the Crimea campaign had been preventable. Farr was impressed with *Notes,* and much of Nightingale's work found its way into the statistical charts and diagrams he prepared for the final report of the Royal Commission. As part of her "flank march" against the forces of resistance to medical reform, Nightingale had the statistical section of the report printed as a pamphlet and distributed widely in Parliament, the government and the army. She even had a few copies of the diagrams framed for presentation to officials in the War Office and in the Army Medical Department.

Nightingale's efforts were not in vain. Four subcommissions were established to carry out the reforms recommended in the report of the Royal Commission. The first presided over physical alterations to military barracks and hospitals: improvements in ventilation, heating, sewage disposal, water supply and kitchens. Other subcommissions drafted a sanitary code for the army, established a military medical school and reorganized the army's procedures for gathering medical statistics.

Nightingale next turned her attention to the health of soldiers in India. She and Farr began to study the sickness and mortality records of the India Office, and she sent inquiry forms to the various British stations in India for information on sanitary conditions there. In 1858 and 1859 she lobbied successfully for the establishment of another Royal Commission to look into the Indian question. Two years later she submitted to the commission a report, based on the inquiries sent to the stations in India, on the conditions that were causing among the troops in India a death rate six times higher than the rate among civilians in England: defective sewage systems, overcrowding in the barracks, lack of exercise and inadequate hospitals, among other things. The commission completed its own study in 1863. After 10 years of sanitary reform, in 1873 Nightingale reported that mortality among the soldiers in India had declined from 69 to 18 per 1,000.

Statistics, as Nightingale so effectively demonstrated, provide an organized way of learning from experience, and medical statistics can teach far more than the simple fact that unsanitary conditions kill. Uniform and accurate hospital statistics, she wrote, would "enable the value of particular methods of treatment and of special operations to be brought to statistical proof"; in short, statistics would lead to improvement in medical and surgical practice. The problem was that the statistics kept by hospitals in Nightingale's day were neither uniform nor consistently accurate. To remedy this she developed, with the aid of Farr and other physicians, a Model Hospital Statistical Form. The form was approved at the International Congress of Statistics, held in London in the summer of 1860.

The new scheme set out the basic categories of data that hospitals should collect: the number of patients in a hospital at the beginning and end of a year and the number of patients admitted during the year, the number of patients who had recovered or who had been either discharged as incurable or dismissed at their request, the number of patients who had died and the mean duration of hospital stays. Yet although the ideal of gathering uniform hospital statistics was clearly a good one, and far ahead of its time, the new scheme was never put into general practice. The proposed form itself was overly complex, and it included an idiosyncratic system for the classification of diseases devised by Farr with which many pathologists strongly disagreed. In medical science, unfortunately, Nightingale did not display the same understanding that led her to recognize the value of medical statistics; for instance, she showed no interest in the new germ theory of disease and its implications for the treatment of contagious diseases.

Nightingale's commitment to statistics transcended her interest in healthcare reform, and it was closely tied to her religious convictions. To her, laws governing social phenomena, "the laws of our moral progress," were God's laws, to be revealed by statistics. Quételet's science, she taught, was "essential to all Political & Social Administration," yet political leaders were for the most part completely untrained in the interpretation of statistics. The result of such ignorance, in Nightingale's view, was legislation that was "not progressive but see-saw-y," written by officials who "legislate without knowing what [they] are doing." That is why she experimented with graphical representations, which everyone could understand, and why she struggled to get the study of statistics introduced into higher education, although her dream of a university chair in statistics did not become a reality until after her death. Even today society has not come around fully to Nightingale's point of view, as is clear from the fact that statistics has yet to become a mandatory part of public education.

Something of the religious fervor Nightingale felt for statistics is evident from her annotation of her copy of Quételet's book *Physique Sociale.* On the title page she incorporated the title into a statement of her own creed:

> The sense of infinite power
> The assurances of solid certainty
> The endless vista of improvement
> from the principles of
> PHYSIQUE SOCIALE
> if only found possible to apply on
> occasions
> when it is so much wanted

To Nightingale, Quételet was the founder of "the most important science in the whole world," because "upon it depends the practical application of every other [science]." Judging from their correspondence, the respect seems to have been mutual.

Although statistics were important to Nightingale, during her later years of being "an influential" she by her own account yearned to return to nursing, her chosen profession, her first "call from God." She could not, however, because she lived a good part of her life after her return from the Crimea as an invalid, practically bedridden.

Although Nightingale's poor health may have been related to a fever she contracted while she was in the Crimea, some have suggested that she did not have an organic illness at all, that her invalidism was neurotic or even intentional. In any event confinement to her bedroom, where she received a steady stream of visitors, did not diminish her influence or keep her from establishing the professional status of modern nursing. With money from the Nightingale Fund (almost 50,000 pounds, raised by public subscription to honor "the Popular Heroine") she was able to realize an early goal, founding the Nightingale Training School for Nurses in 1860. She could not, as she had hoped, superintend the school, but it followed her principles: "(1) That nurses should have their technical training in hospitals specially organized for that purpose; (2) That they should live in a home fit to form their moral life and discipline."

Both principles were radical in their time. That they are accepted as commonplace today is testimony to Florence Nightingale's service to nursing, which did as much as any scientific advance to improve the general quality of medical care. In view of her other passion, it is appropriate that another telling indicator of that service is statistical: in 1861 the British census found 27,618 nurses in Britain, and it listed that figure in the tables of occupations under the heading "Domestics"; by 1901 the number had increased to 64,214, and it was listed under "Medicine."

Source: *Scientific American*, March 1984

Chance encounters

Two strangers from opposite sides of the United States sit next to each other on a business trip to Milwaukee and discover that the wife of one of them was in the tennis camp run by an acquaintance of the other's. This sort of coincidence is surprisingly common. If we assume each of the approximately 200 million adults in the United States knows about 1,500 people, and that these 1,500 people are reasonably spread out around the country, then the probability is about one in a hundred that they will have an acquaintance in common, and more than ninety-nine in a hundred that they will be linked by a chain of two intermediates.

We can be almost certain, then, given these assumptions, that two people chosen at random will be linked, as were the strangers on the business trip, by a chain of at most two intermediates. Whether they'll run down the 1,500 or so people they each know (as well as the acquaintances of each of these 1,500) during their conversation and thus become aware of the two intermediates linking them is another, more dubious matter.

These assumptions can be relaxed somewhat. Maybe the average adult knows fewer than 1,500 other adults, or, more likely, most of the people he or she does know live close by and are not spread about the country. Even in these cases, however, the probability of two randomly selected people being linked by two intermediates is unexpectedly high.

A more empirical approach to coincidental meetings was taken by psychologist Stanley Milgram, who gave each member of a randomly selected group of people a document and a (different) 'target individual' to whom the document was to be transmitted. The directions were that each person was to send the document to the person he knew who was most likely to know the target individual, and that he was to direct that person to do the same, until the target individual was reached. Milgram found that the number of intermediate links ranged from two to ten, with five being the most common number. This study is more impressive, even if less spectacular, than the earlier a priori probability argument. It goes some way toward explaining how confidential information, rumors, and jokes percolate so rapidly through a population.

If the target is well known, the number of intermediates is even smaller, especially if you have a link with one or two celebrities. How many intermediates are there between you and President Reagan? Say the number is N. Then the number of intermediates between you and Secretary General Gorbachev is less than or equal to $(N + 1)$, since Reagan has met Gorbachev. How many intermediates between you and Elvis Presley? Again, it can't be bigger than $(N + 2)$, since Reagan's met Nixon, who's met Presley. Most people are surprised when they realize how short the chain is which links them to almost any celebrity.

When I was a freshman in college, I wrote a letter to English philosopher and mathematician Bertrand Russell telling him that he'd been an idol of mine since junior high school and asking him about something he'd written concerning the German philosopher Hegel's theory of logic. Not only did he answer my letter, but he included his response in his autobiography, sandwiched between letters to Nehru, Khrushchev, T. S. Eliot, D. H. Lawrence, Ludwig Wittgenstein, and other luminaries. I like to maintain that the number of intermediates linking me to these historical figures is one: Russell.

Another problem in probability illustrates how common coincidences may be in another context. The problem's often phrased in terms of a large number of men who check their hats at a restaurant, whereupon the attendant promptly scrambles the hat-check numbers randomly. What is the probability that at least one of the men will get his own hat upon leaving? It's natural to think that if the number of men is very large, this probability should be quite small. Surprisingly, about 63 percent of the time, at least one man will get his own hat back.

Put another way: If a thousand addressed envelopes and a thousand addressed letters are thoroughly scrambled and one letter is then placed into each envelope, the probability is likewise about 63 percent that at least one letter will find its way into its corresponding envelope. Or take two thoroughly shuffled decks of cards. If cards from each of these decks are turned over one at a time in tandem, what is the probability that at least one exact match will occur? Again, about 63 percent. (Peripheral question: Why is it necessary to shuffle only one of the decks thoroughly?)

A very simple numerical principle that's sometimes of use in accounting for the certainty of a particular kind of coincidence is illustrated by the mailman who has twenty-one letters to distribute among twenty mailboxes. Since 21 is greater than 20, he can be sure, even without looking at the addresses, that at least one mailbox will get more than one letter. This bit of common sense, sometimes termed the pigeonhole or Dirichlet drawer principle, can occasionally be used to derive claims that are not so obvious.

Source: John Allen Paulos (1988) *Innumeracy*

Extract from The Roots of Coincidence

The odds against chance, which the experiments by Rhine and his English followers demonstrated, were indeed astronomical—of the order of millions, and even higher. Thus, according to the rules of the game in the exact sciences, the question 'Does ESP exist?' should have been regarded as settled, and the controversy should have shifted to the next problem, 'How does it work?'

Among English experimenters the most impressive results were achieved by the eminent Cambridge psychologist Thouless, and the mathematician Dr Soal.

And yet the malaise persisted. For one thing, guessing card after card a hundred, a thousand times is a very monotonous and boring exercise; even the most enthusiastic experimental subjects showed a marked decline in hits towards the end of each session, and after some weeks or months of intense experimenting most of them lost altogether their special gifts. Incidentally, this 'decline effect' (from the beginning to the end of a session) was considered as additional proof that there was some human factor at work influencing the scores, and not just chance.

Nevertheless there was, as already said, something profoundly unsatisfactory in the experimental design to all but the mathematically minded. An example will illustrate this. A subject in an ESP test makes a series of a hundred successive guesses at a hundred consecutive cards (which the experimenter turns up one by one in a different room or a different building). Since there are five types of cards, his chance expectation is one correct guess in five or twenty correct guesses in a hundred tries. Assuming he has made twenty-two, instead of twenty correct guesses—nobody will turn a hair. The experiment continues until the subject has made a thousand guesses—and he again does ten per cent better than chance expectation: two hundred and twenty hits instead of two hundred. Here, as the universally accepted probability calculus (based on the so-called binomial formula) shows, the odds against such a result occurring by pure chance are six to one. The subject carries on to five thousand guesses, and continues to score ten per cent over average: eleven hundred hits instead of a thousand. The odds against chance are now two thousand against one. Relentlessly he carries on until he has made ten thousand guesses—and lo, he scored two thousand two hundred instead of two thousand hits. The odds for this being the work of pure chance are now one in two million.

Such is the 'law of great numbers'. To the mathematician and physicist it is an elementary tool; to the non-mathematician the steep rise of the odds against chance is a paradox and an added source of intellectual discomfort. The nearest one can get to an intuitive grasp of the paradox is by reflecting that if that ten per cent deviation from average, however trivial in itself, keeps stubbornly persisting on and on to a thousand, five thousand, ten thousand tries, then it stands to reason that there must be a reason for it. And that is all that the probability calculus is meant to prove. The first published results by Rhine in 1934 contained the complete record of eighty-five thousand card-calling tries, conducted with a number of selected subjects. The overall score averaged twenty-eight hits instead of twenty in a hundred guesses. The odds against this are, as already said, astronomical, and this was in fact the first important break-through of ESP into respectability.

The record included the scores of subjects who had been rejected after a preliminary try because their scores were average or *below*.

And yet there is to the non-mathematician something profoundly disturbing in the idea that an average of twenty-eight correct guesses instead of twenty should have such momentous results, even when very large numbers are involved. The mathematically naive person seems to have a more acute awareness than the specialist of the basic paradox of probability theory, over which philosophers have puzzled ever since Pascal initiated that branch of science (for the purpose of improving the gambling prospects of a philosopher friend, the Chevalier de Méré). The paradox consists, loosely speaking, in the fact that probability theory is able to predict with uncanny precision the overall outcome of processes made up out of a large number of individual happenings, each of which in itself is unpredictable. In other words, we observe a large number of uncertainties producing a certainty, a large number of chance events creating a lawful total outcome.

But, paradoxical or not, it works. In thermodynamics we can predict exactly the temperature of a gas under a given pressure, although the gas molecules, whose speed determines the temperature, all fly about, collide and rebound in their crazy ways like a swarm of gnats on an LSD trip. The archaeologist who determines the age of a fossil by the radio-carbon test relies on the fact that radio-active substances decompose at a rigorously fixed rate (their so-called 'half-life'), although the disintegration of their individual atoms is spontaneous and unpredictable even in theory. In sub-atomic physics in general, Heisenberg's Uncertainty Principle and the laws of quantum mechanics have replaced causality by probability. In genetics, ever since Abbot Mendel started counting his dwarf peas, the statistical approach reigns supreme. And so it does in the more mundane spheres of the insurance business and gambling casinos. None of them could survive if the laws of chance were not so paradoxically reliable.

[The 'half-life' is] the time it takes for half of the atoms of a given radio-active substance to decay.

A classical example of statistical wizardry concerns the death of soldiers kicked by cavalry horses in the German Army from 1875 to 1894. The total number of deaths in fourteen army corps over these twenty years 196. A German mathematician undertook to calculate from these data alone the theoretical frequency of zero, one, two or more deaths per army corps per year. The comparison between theoretical and actual figures reads:

He used the so-called Poisson distribution, derived from the more widely used Gaussian curve.

Number of deaths per army corps per year	*Actual number of instances*	*Theoretical number of instances*
0	144	139.0
1	91	97.3
2	32	34.1
3	11	8.0
4	2	1.4
5 or more	0	0.2

To make this somewhat involved table clearer: how often in these twenty years would any of the fourteen army corps suffer two casualties in a single year? The theory says that this should occur 34.1 times. In fact, it occurred thirty-two times. All the mathematician had to go on for his calculations was the total number of casualties in $14 \times 20 = 280$ 'army corps years'. From this single datum he was able to deduce with the aid of Poisson's equation the relative frequency of 0, 1, 2, 3 or 4 casualties suffered by a single army corps in a single year.

Another mystery of the theory of chance is reflected in the following quotation from Warren Weaver:

> The circumstances which result in a dog biting a person seriously enough so that the matter gets reported to the health authorities would seem to be complex and unpredictable indeed. In New York City, in the year 1955, there were, on the average, 75.3 reports per day to the Department of Health of bitings of people. In 1956 the corresponding number was 73.6. In 1957 it was 73.2. In 1957 and 1958 the figures were 74.5 and 72.6.

Weaver comments:

> One of the most striking and fundamental things about probability theory is that it leads to an understanding [*sic*] of the otherwise strange fact that events which are individually capricious and unpredictable can, when treated en masse, lead to very stable *average* performances.

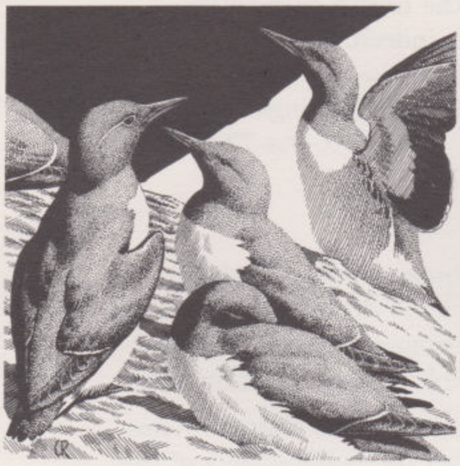
But does it really lead to an *understanding*? How do those German Army horses adjust the frequency of their lethal kicks to the requirements of the Poisson equation? How do the dogs in New York know that their daily ration of biting is exhausted? How does the roulette ball know that in the long run zero must come up once in thirty-seven times, if the casino is to be kept going? The soothing explanation that the countless minute influence on horses, dogs or roulette balls must in the long run 'cancel out', is in fact begging the question. It cannot answer the hoary paradox resulting from the fact that the outcome of the croupier's throw is not causally related to the outcome of previous throws: that if red came up twenty-eight times in a row (which, I believe, is the longest series ever recorded), the chances of it coming up yet once more are still fifty-fifty.

Probability theory is the offspring of paradox wedded to mathematics. But it works. The whole edifice of modern physics relies on it, the geneticist relies on it, the archaeologist relies on it, business relies on it. And it works, to say it once more, with uncanny accuracy where large numbers of events are considered *en masse*. That precisely is the reason why, when a large series of events persistently deviates from chance expectation, we are driven to the conclusion that some factor other than chance is involved.

Source: Arthur Koestler (1974) *The Roots of Coincidence*

Guillemot

Uria aalge



Picture of guillemots

The constant streams of adults arriving at and departing from sheer cliffs or from cascades of boulders at their feet, and the loud 'murrings' of these noisy birds, make a visit to a Guillemot colony one of the more memorable of ornithological experiences. This is a common seabird and although the largest colonies tend to be in the more isolated and wilder parts of the country, where the habitat is suitable Guillemots breed relatively close to centres of human population.

The Guillemot is a marine species, eschewing even brackish water. Its preferred nesting habitats are ledges, sheer cliffs or boulders at their bases, and the tops of isolated stacks. Britain and Ireland lack the large colonies on flat, low islands found elsewhere. *Status of Seabirds* gives median colony size in England, Wales, and Ireland as 100 birds whereas in Scotland the figure is 400–500. Most of the birds, however, breed in much larger assemblies.

Colonies are sited where suitable nesting habitats occur close (say within 50 km) to good feeding areas. Guillemots feed their young on single fish, which entails much toing and froing, so birds nest as close as possible to their food. Hence, the largest colonies tend to be where there is a good feeding area close to a limited land area, as at the ends or corners of islands or island chains, e.g. Hermaness, Foula, Noss and Sumburgh Head in Shetland and islands off otherwise unsuitable coasts, as at Britain's largest colony on Handa (98,700 birds, SCR). Mainland sites are quite acceptable (Fowlsheugh in Grampian, Bempton in Humberside) as long as the cliffs give protection from ground predators.

Scotland has most of the Guillemots in Britain and Ireland and SCR recorded 28 individual colonies with more than 10,000 birds (the unit used in Guillemot surveys). In N and E Scotland Guillemots breed on most suitable cliffs, but in England the only sizeable concentrations are in the north and east, and none breed between Flamborough head and the Isle of Wight, presumably due (partly, at least) to lack of suitable habitat.

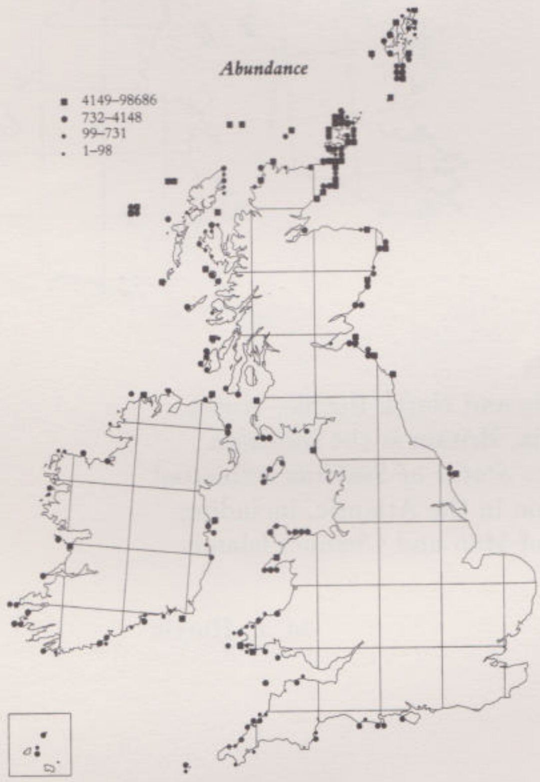
Colonies in SW England and Wales are smaller, but Ireland has four well dispersed large colonies.

Guillemot colonies are traditional and many have been known for centuries, having been used as sources of eggs, flesh and feathers. Despite

the apparent gain or loss of a few minor colonies, there has been little change in distribution in recent years.

The *68–72 Atlas*, reporting on the total population, noted 'heartening signs of stability, or even of an increase despite occasional pollution calamities'. This optimism was well founded. Counting Guillemots is difficult, and single counts give inaccurate estimates of population. The SCR surveys in 1985–87 found that the total British and Irish population had approximately doubled since the Operation Seafarer estimate in 1969–70. The population of SW Scotland increased by 380% and even those of Wales and SW England, areas about which the *68–72 Atlas* had been pessimistic, had increased by 130% and 65% respectively.

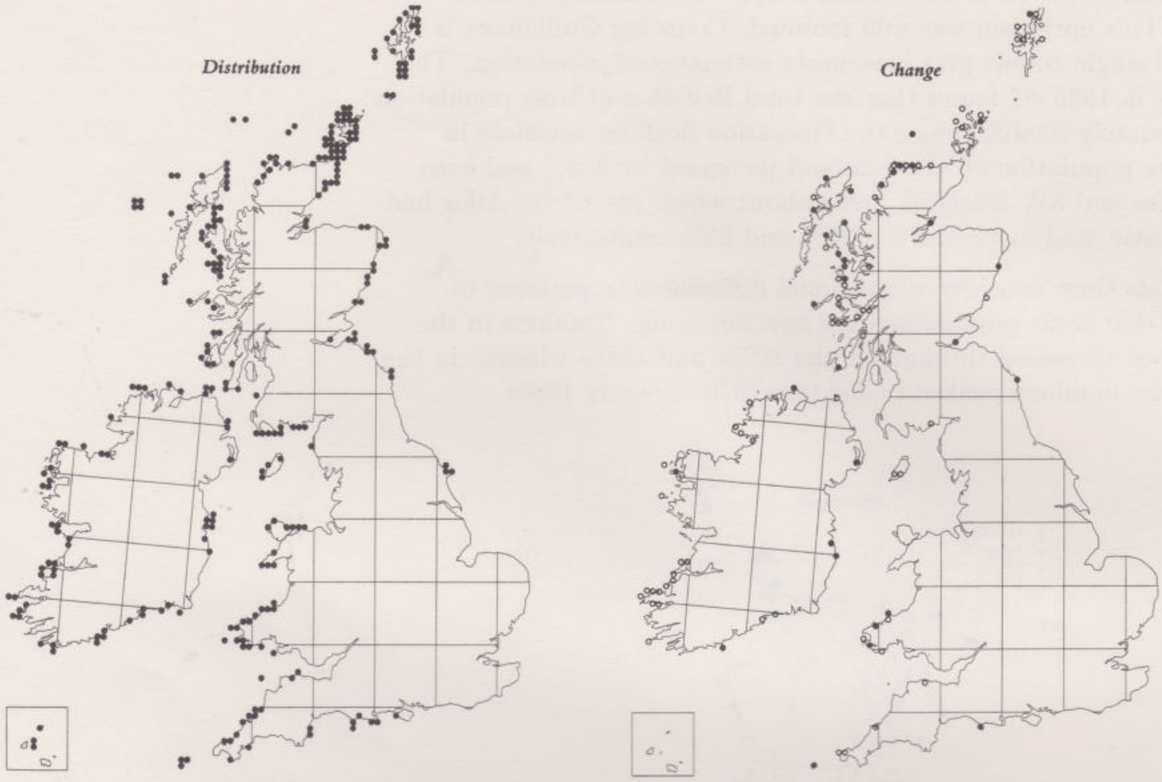
Regular counts show considerable regional differences in patterns of change, and that some populations are now declining. Numbers in the south and west increased throughout the 1970s and 1980s whereas in the north and east numbers peaked in the late 1970s or early 1980s.



In the North Sea, decreases started in the far north and spread towards the south, and there is a significant relationship between latitude and the rate of decrease, the numbers at northern colonies declining more rapidly (Harris 1991). Breeding success and adult survival have remained high, but there has been a marked reduction in the number of immatures returning to colonies (Harris and Wanless 1988).

Oiling, shooting, and drowning in fishing nets, are obvious causes of death: chemical poisoning, disease and starvation are less so, but could well be critical. There are large regional and temporal differences in the causes of mortality (Mead 1989) making it difficult to decide which factor(s) might have caused the widespread increases and some subsequent declines in the last 20 years. Change in food availability is a strong possibility, and there is a negative link between the numbers of sprats (an important prey) in the North Sea and the first winter mortality of Guillemots (Harris and Bailey 1992). Many more auks have been seen in the southern North Sea in recent

years, suggesting a change in winter distribution which matches a southern shift of sprats (Camphuysen 1989). Only detailed population studies can hope to discover the factors influencing the numbers of these seabirds.



Guillemots breed widely in the North Atlantic and North Pacific. Apart from a few hundred birds in France and Iberia, Britain is the southern limit of the species' range in the NE Atlantic. *Status of Seabirds* estimated 5–6 million birds in the Pacific and 3–5 million in the Atlantic, including 950,000 in Scotland, 63,000 in England, Isle of Man and Channel Islands, 34,000 in Wales and 153,000 in Ireland.

M. P. Harris

Years	Breeding evidence		
	Br	Ir	Both (+CI)
1968–72	225	74	302
1988–91	212	59	274
% change	−5.8	−20.3	−9.3

Source: David Wingfield Gibbons, James B. Reid, Robert A. Chapman (eds) (1993) *The New Atlas of Breeding Birds in Britain and Ireland: 1988–1991*

EXTRA
GEOGRAPHY

Blindly into the ditch

The new Order has brought Eckert's map out of obscurity . . . and entirely missed the point. **David Wright** reports

There is an extraordinary error in the world maps in the new geography national curriculum booklet. The "North" arrow does not point North; it actually points North-east. South points South-west.

This is the geographical equivalent of 2x2=5. If we cannot get North and South correct, everthing else in geography is liable to be distorted as well. These world maps, on the Eckert IV projection, have unwisely been published without lines of longitude, and the North arrow has been printed vertically, without regard to these lines. But the essence of the Eckert IV map is that lines of longitude are increasingly curved towards the edges of the map. The North arrow *must* follow a line of longitude — but in this case does not do so, with disastrous consequences.

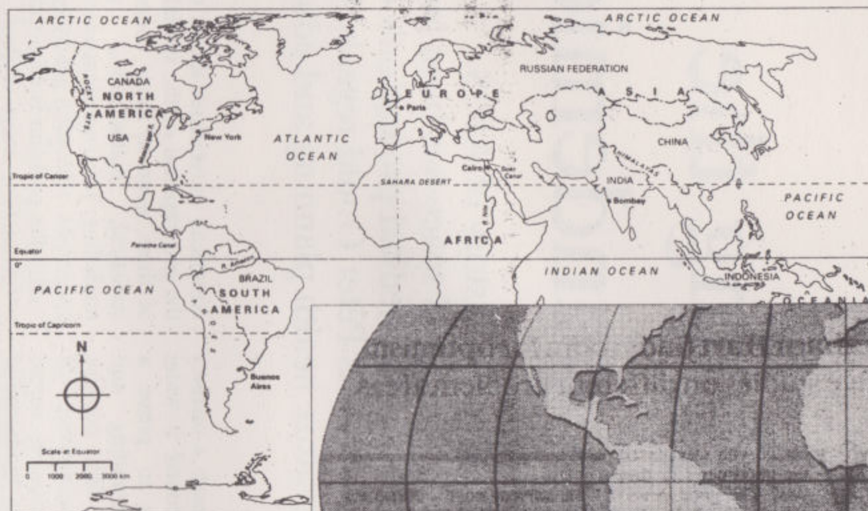
Why was this little-known map projection chosen? It dates from 1906, and lay in well-deserved obscurity for 88 years — even the creator of the map was not enthusiastic about it, preferring his Eckert VI projection which has sinusoidal meridians. But Eckert VI seems to have sunk without trace.

In 1568 they knew a globe was best

Eckert IV is not a *bad* projection — at least, not until the spectacular error was added to it for the revised Order. It is equal-area, and is certainly better than its predecessor, the Modified Gall projection. And it is infinitely better than the Peters projection, with its very confusing and distorting shapes.

But Eckert IV has major weaknesses. The main problems are the distortions at the "corners", caused by the curved lines of longitude. There is also considerable elongation in the tropics: enough to make a difference, but not enough to be immediately recognisable. In one sense, therefore, the map is more misleading than Peters. Most people know that shapes on Peters are wrong; most people will assume that Eckert is right. Yet Africa in the real world is as "wide" as it is "long"; on the Eckert map Africa is too long and too narrow.

How strange that there should be a more accurate world map on your Barclaycard bill than in the national curriculum! The Interrupted Sanson-Flamsteed map which Barclaycard uses is equal-area, with correct shapes as well. It is also elegant and easy for children of all ages to understand, because it is the nearest thing to "unpeeling the skin" of a globe.



Above: Eckert IV in the national curriculum Order, with the North arrow facing North-east; right: with the North arrow placed on the lines of longitude

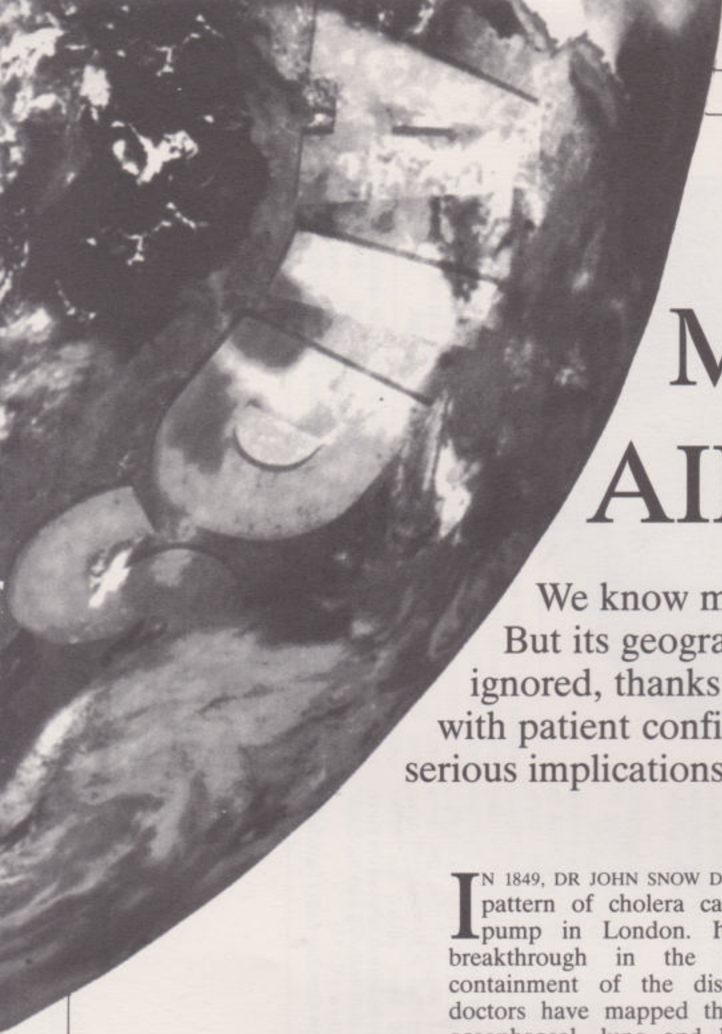
This is the second major error on national curriculum world maps. In 1992 they described Gall's map, with its huge Greenland, as "equal-area". At the time I wrote: "If they can't get the *map* right, what *can* they get right?" (*TES* 10.4.1992), and I suggested that the Revd James Gall, if he were still alive, would have quoted the Bible: "They be blind leaders of the blind; they shall both fall into the ditch" (Matthew 15, v14).

This time they really have fallen into the ditch — and we will all follow unless we start to make sense of our globe. This suggests that something much more serious than a mere gremlin is wrong. Could it be that Geographers have simply omitted to study the globe as a whole, for the past 20 years or so, and so today's top geographical curriculum experts cannot spot a fundamental error?

If so, I suggest that we need a major focus on globe education — for "experts", for teachers and for children of all ages. A globe focus can be fun and it is not difficult — but it really is vital.

David R Wright is author of Children's Atlas *and* Environment Atlas *(Philips/WWF), and is a fellow of the University of East Anglia*

Source: *Times Educational Supplement*, 31 March 1995



Mapping the AIDS pandemic

We know much about the history of AIDS. But its geography in place and space has been largely ignored, thanks in part to the medical profession's concern with patient confidentiality. As Peter Gould reveals, this has serious implications for future health planning and education

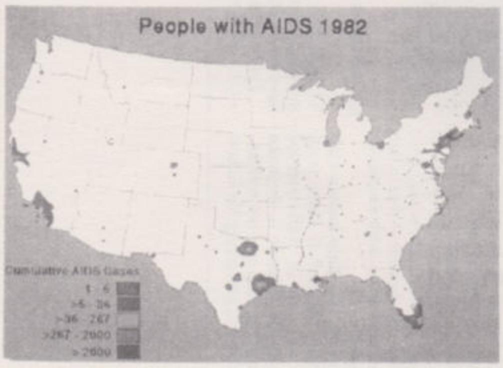
In 1849, Dr John Snow discovered a distinct pattern of cholera cases around a water pump in London. It was to prove a breakthrough in the understanding and containment of the disease. This century, doctors have mapped the concentrations of oesophageal, lung and liver cancers across China. Maps have been used as fundamental tools of science throughout history. Distinct geographic patterns of diseases always raise questions about their cause, and often suggest possibilities for intervention, and are essential for effective medical planning and preventive education.

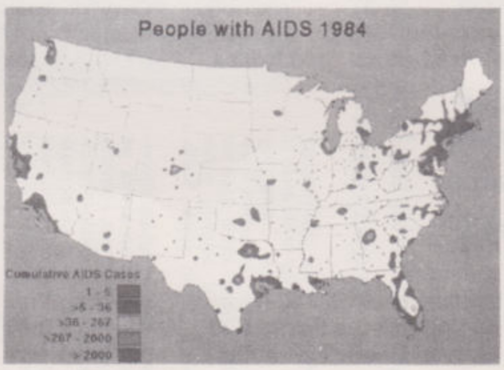
So why, in the first dozen years of the AIDS pandemic, has the map virtually disappeared from use? Perhaps even more importantly, why is there so little geographic thinking and awareness behind this issue? Why does thinking always move to the "when" questions along the dimension of time, but seldom to the "where" questions lying in the dimensions of space?

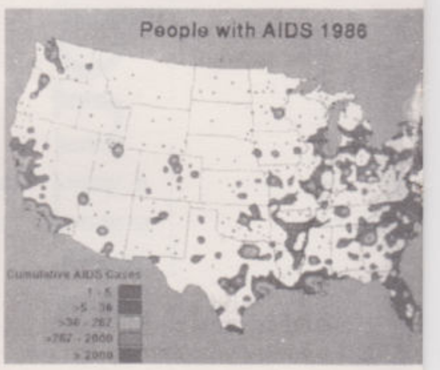
A recent examination of doctoral programmes in epidemiology at major American universities shows that no sense of the geographical or the spatial emerges in any of them, even in those courses dealing with statistical, mathematical and computer modelling. This owes much to the poor state of geographic education generally in the US.

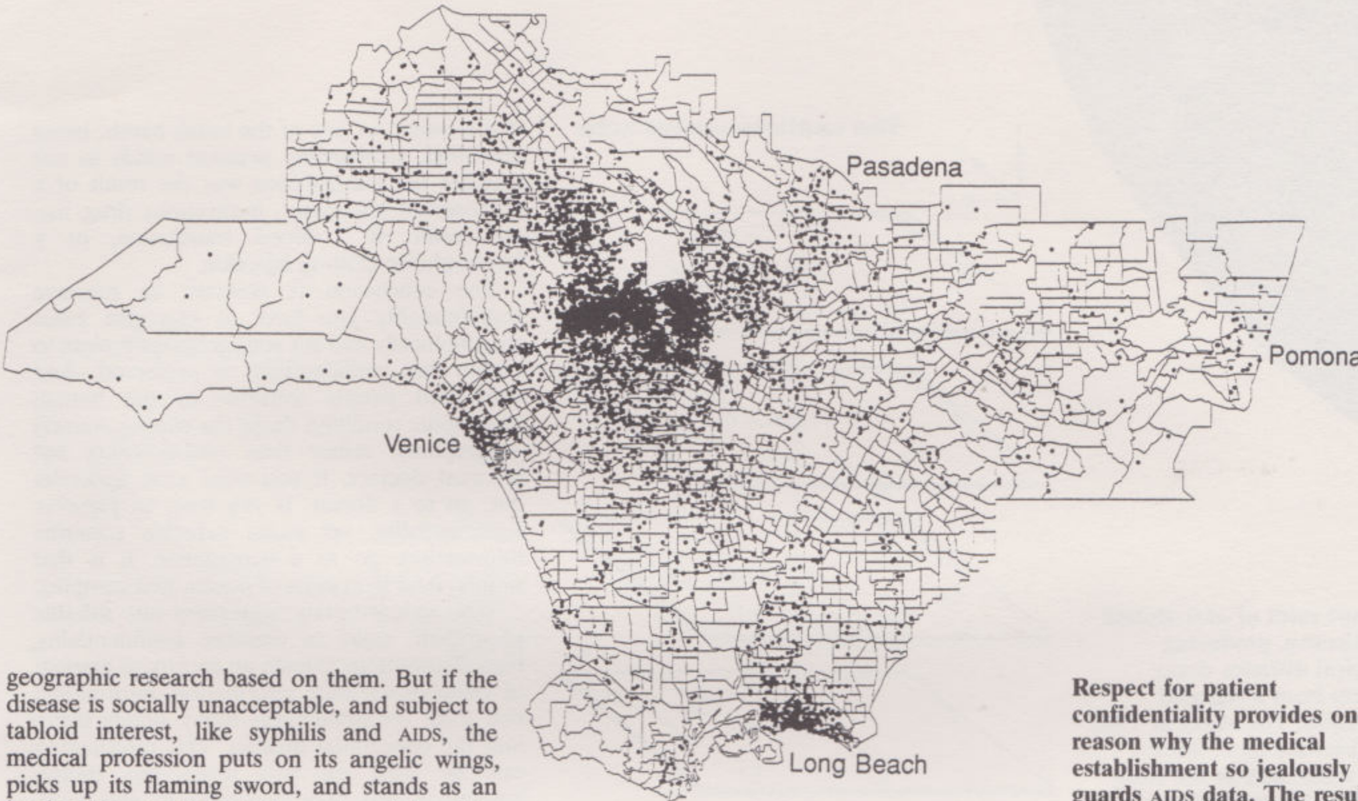
But the problem is even more complex and, unfortunately, more delicate, owing to the absolute power that the medical profession exercises over patient information. The medical profession, with its vast modern bureaucracies, regards itself as the guardian of human health. Supported by the Hippocratic Oath, it assumes a noble and selfless public posture cloaked with ethical righteousness. It has established control over valuable scientific information, even if it does not know what to do with it. Much of this information is purely geographic. In essence, it specifies the where of a particular patient with a particular illness.

If a patient happens to be a child with leukaemia or measles, no problem seems to arise. We have lots of maps of childhood leukaemia and measles, and extraordinarily fine









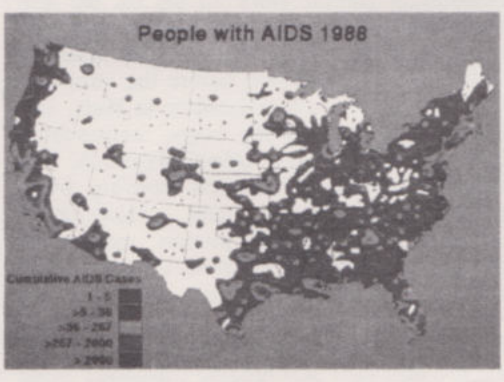
geographic research based on them. But if the disease is socially unacceptable, and subject to tabloid interest, like syphilis and AIDS, the medical profession puts on its angelic wings, picks up its flaming sword, and stands as an implacable guardian at the gates of the data bank.

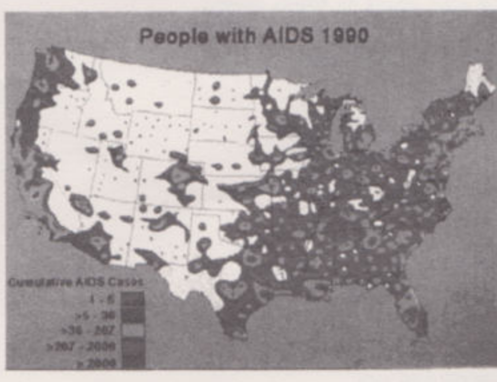
While it is entirely right that a person's medical records should remain confidential, the problem with this is that the question of confidentiality, particularly as it pertains to scientific inquiry, has nothing to do with medicine, and everything to do with geography. Whether a patient's confidentiality is preserved or not depends solely on the underlying human geography of the region, the very domain of thinking where the medical profession has no expertise whatsoever. It exerts unreasoned power on the grounds that it is better to be safe than sorry.

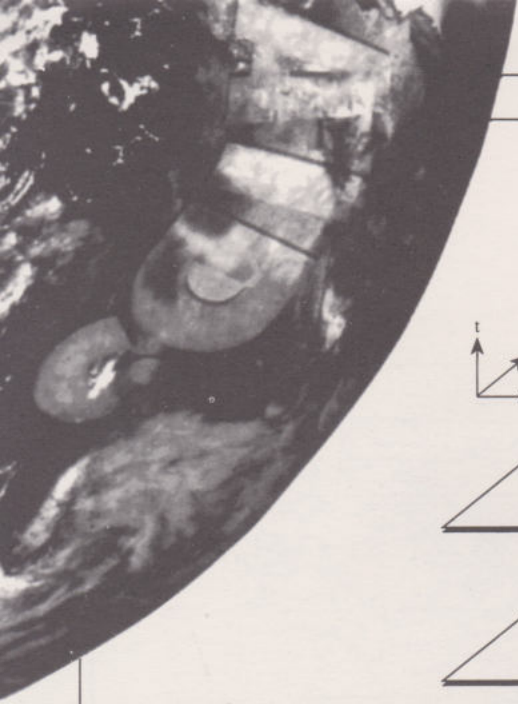
At Northridge, California, students under the guidance of Dr William Bowen, and with the full cooperation of the AIDS Surveillance Unit of Los Angeles, have constructed a series of maps showing the spread of AIDS from 1983 to 1989 in the black, white and Hispanic groups of that city. In a densely settled area like Los Angeles, each case can be represented by a dot placed at random within one of the 1,600 or so census districts. In this way, a reasonably accurate and geographically fine-grained picture can be built up of the way the Human Immuno-deficiency Virus (HIV) and AIDS have spread, without any loss whatsoever of patient confidentiality.

But would such mapping work in central Wyoming, where a single cattle ranch might be the size of several hundred of Los Angeles' census districts? Obviously not. With only six people, and several hundred head of cattle in the area, a single dot could very easily result in ▶

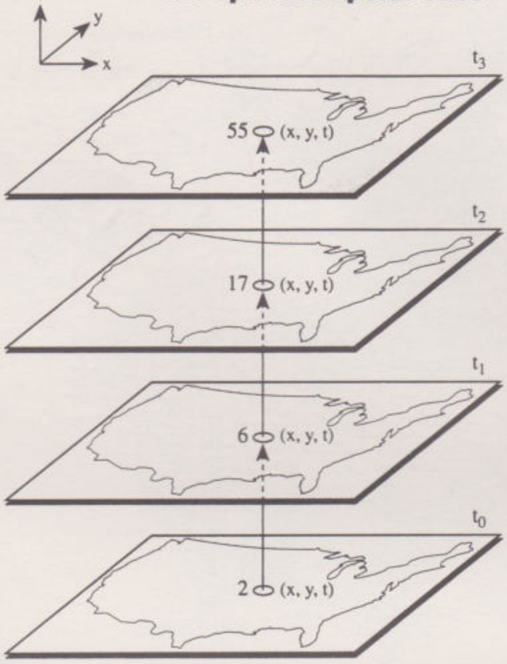
Respect for patient confidentiality provides one reason why the medical establishment so jealously guards AIDS data. The result is poor understanding of the epidemic's geography. But confidentiality is dependent on scale, and who better to judge this than geographers? Maps at a continental scale (below left) offer a clear picture of the geographic spread of the disease while still respecting privacy. Even quite a large-scale map, like that of Los Angeles (above), can show reported AIDS cases by census district without violating privacy





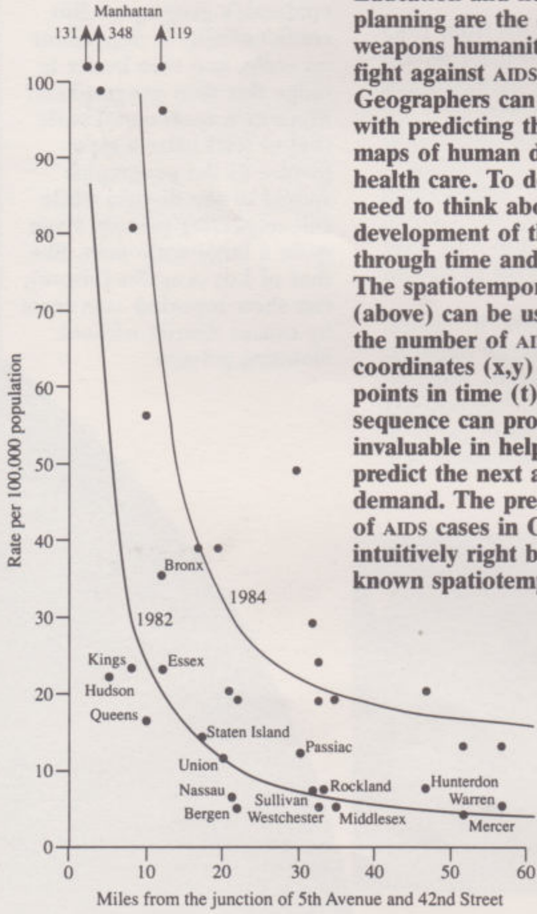


The spatiotemporal cube



Education and health planning are the only weapons humanity has in the fight against AIDS. Geographers can help here with predicting the next maps of human demand for health care. To do so, they need to think about the development of the disease through time and over space. The spatiotemporal cube (above) can be used to show the number of AIDS cases at coordinates (x,y) at different points in time (t). Such a sequence can prove invaluable in helping to predict the next areas of demand. The predicted map of AIDS cases in Ohio seems intuitively right based on the known spatiotemporal data

Below: rates of AIDS around Manhattan, producing classical distance decay effects from a regional epicentre. The effect in 1982 has become more intense two years later as the wave of infection strengthens



◀ the owner, or one of the ranch hands, being identified, so allowing prurient minds to ask whether the transmission was the result of a homosexual encounter, intravenous drug use, the result of a blood transfusion, or a haemophiliac clotting injection.

The conclusion is obvious: to preserve confidentiality you have to aggregate cases geographically and cut your geographic cloth to ensure that confidentiality is preserved. And you need people sensitive to the human geographic condition doing the cutting, namely geographers, rather than well-meaning but ignorant doctors. If you want your appendix out, go to a doctor. If you want to preserve confidentiality, yet retain valuable scientific information, go to a geographer. It is that simple. And in matters of power, that complex.

With data properly aggregated into suitable geographic areas to preserve confidentiality, map distributions contain an enormous amount of valuable scientific information, information that most epidemiologists today simply throw into the conceptual dustbin. This information can be used on two levels—the simple, immediate, but very important graphic level, and the advanced analytical level.

The problem Britain faces in compiling this sort of information is that the health areas for reporting medical statistics are simply ludicrous (there is no polite way of putting it) for scientific work. One region stretches from Land's End to the Avon, lumping Plymouth, Exeter, Bristol and other cities with the granite outcrops of the National Trust on Dartmoor. As for Scotland, what does a single number mean when it lumps Edinburgh and Glasgow, two of the peaks on the AIDS surface, with the Isle of Skye and the Outer Hebrides?

But when a virus producing close to 100 per cent mortality starts to move through a population, the question of where it is becomes just as important as when it arrives. Geographers and epidemiologists must become people who refuse to separate space and time. Even simple map sequences can tell a devastating graphic story. In the US, we now know that there were at least 1,485 people who already had the various infections characteristic of HIV by 1982, the year before the virus was identified at the Pasteur Institute in Paris. AIDS had already reached the major metropolitan centres of New York, Miami, Houston, Los Angeles and San Francisco, moving by a process geographers call "hierarchical diffusion", carried by human movements strongly controlled by relations between major centres in the urban hierarchy.

Two years later, these urban areas had become major regional epicentres, pumping out the HIV into their surrounding commuter fields

by a process of spatially contagious diffusion, while smaller centres continued to receive the virus as it jumped down the urban hierarchy. Even by 1982, classical distance decay effects could be seen around major cities—if any responsible person had thought about them at the time—and they have continued to intensify to this day. In New York, Dr Rodrick Wallace has shown how rates of AIDS in the larger metropolitan region are strongly related to the intensity of commuting, and how the acceleration of the epidemic increases sharply from year to year.

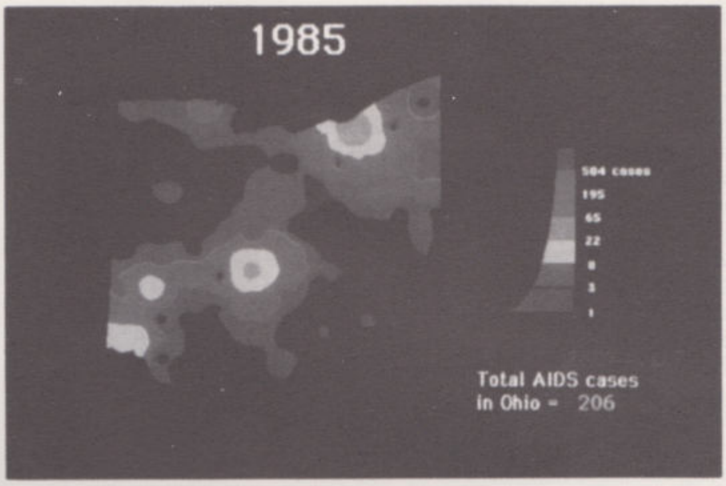
By 1986, the devastating effects of hierarchical and spatially contagious diffusion produced an intensifying and thickening pattern related to major transport routes: the old Magistral between New York and Chicago, the Interstate 95 all the way from Boston to Miami, and on the west coast the emergence of Interstate 5 and Route 101 joining the northern and southern borders of the country. The latter alignment particularly intensifies by 1988, and by 1990 most of the eastern half of the country, the entire west coast, and major western urban centres have people converting from the infected condition to full-blown AIDS.

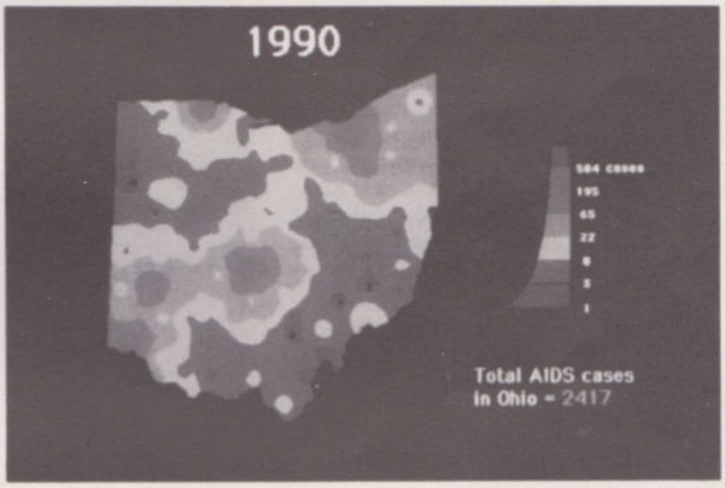
The two ways of looking at geographic information—graphically and analytically—are not unrelated. If you were given a blank map of the US, and you ran your eye carefully along that sequence of AIDS in America, you could make a pretty good stab at predicting what the next map would look like. AIDS does not develop across geographic space in some sort of random way. On the contrary, that sequence of maps seems to have a great deal of "spatial logic" to it. We have some sort of expectation about what the next map will look like before we actually see it.

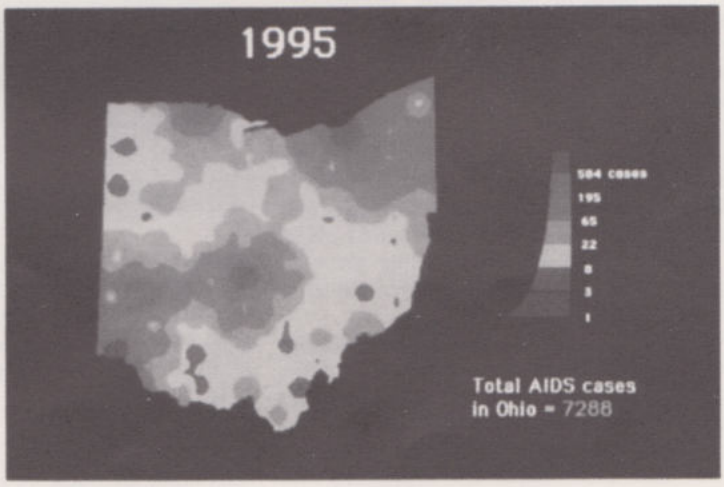
Good science is often a matter of turning such intuition into well-defined and computable ways of predicting, and this is where advanced analytical approaches make their appearance. Yet no matter how large and fast the computers, and no matter how complex the computer algorithms, all useful predictions need to use the geographic information we all feel intuitively.

All sorts of sciences use sophisticated ways of analysing time series—sequences of numbers along the single dimension of time. Measurements in the form of numbers are plotted on pieces of graph paper or computer screens, and scientists look for some sort of order or structure to them. Things that happen quite regularly over time allow them to make predictions.

Geographers insist that in the same way this sort of information may be seen over time, so information may be discovered and analysed ▶







◀ over space. And in the same way that scientists use rather sophisticated approaches to detect informative patterns in time series, so geographers have ways of finding valuable information in spatial series (map distributions). A series of maps showing something diffusing over space and through time is known as a spatiotemporal series. These are basically a sequence of maps showing how the AIDS epidemic is intensifying over space and time. Each number represents the people with AIDS in a particular area, whose centre is (x,y), at a particular time (t).

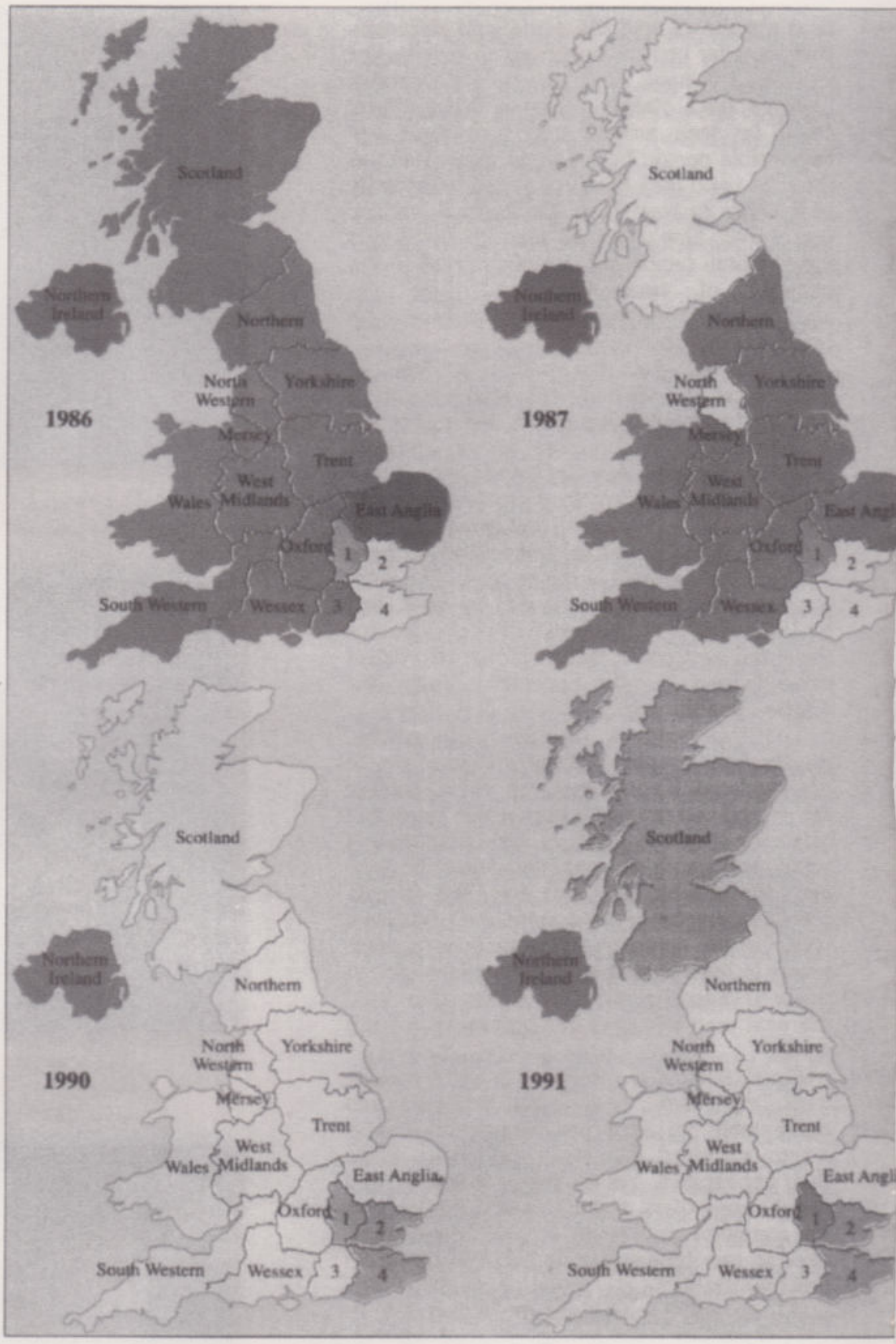
The trick is not to follow one place (x,y) along the vertical time axis (t) because that would take us straight back to the conventional time series analysis, with the epidemiologist churning out single numbers that no-one can do anything with. Similarly, we do not want to look at just a single slice along the spatial axes (x,y) because that would put us back with the conventional geographers doing static map descriptions. What we have to do is take the entire spatiotemporal cube at one gulp, and then use the historical information of the time series, and the geographical information of the spatial series, to predict the next maps.

In the middle of a mortal pandemic, no one tackles the problem of predicting the next maps of AIDS out of a sense of detached intellectual curiosity. Predictions like these must be of some use, and they must be judged on purely utilitarian grounds. What we have is a killer viral disease for which we have no cure. A recent Anglo-French study indicated that AZT had no more effect on retarding the progress of HIV infection than a placebo. So what can we do? What we can do is expand and prepare our medical delivery systems to take care of mortally ill people in decent and humane ways.

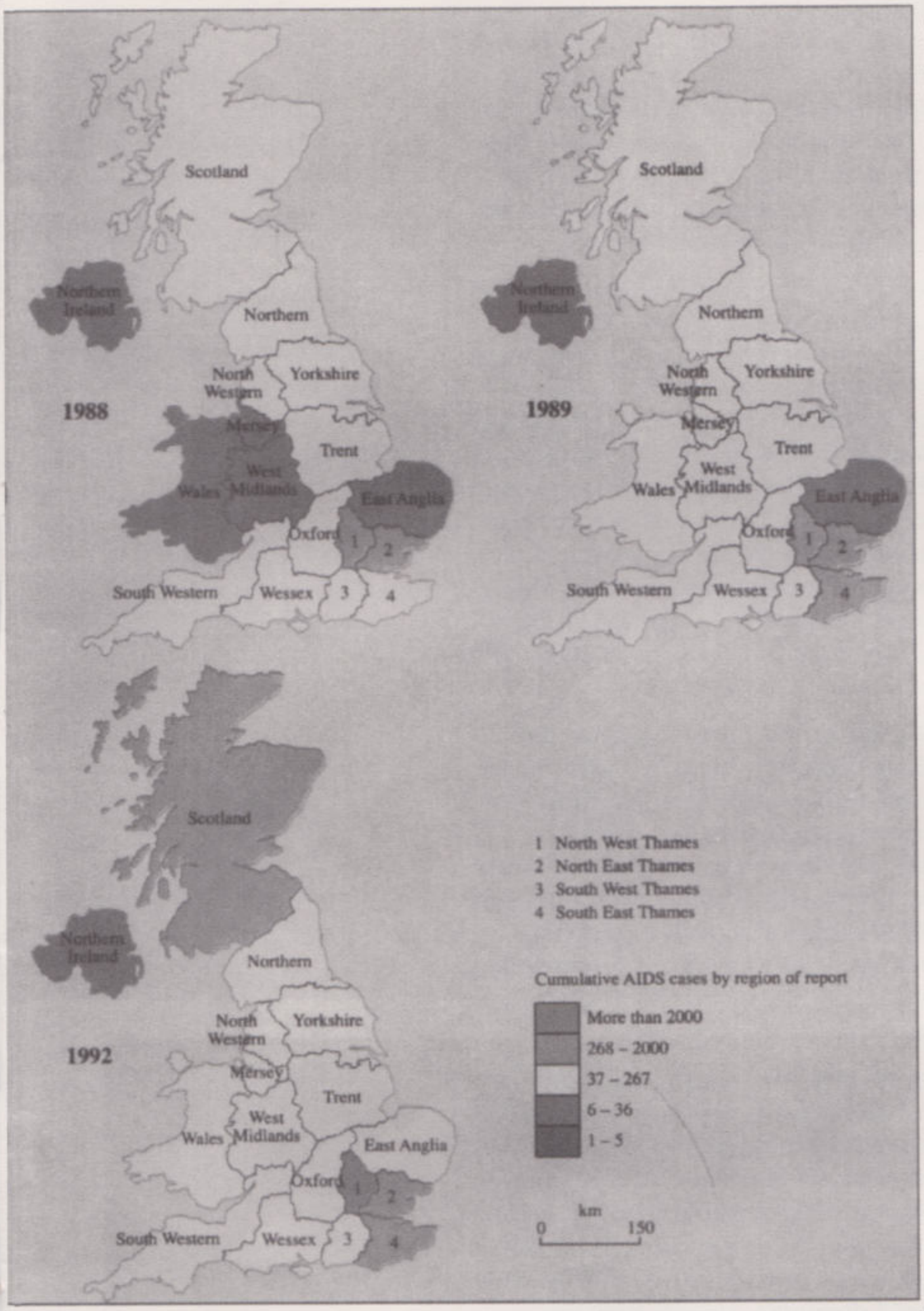
All health planning along the time line into the future must also take place in space. The where of the geographer is crucial. In America, some urban hospitals are already being overwhelmed, and the situation will get worse. Expanded facilities are needed, but the where question must be answered in a particular geographic context. There is always a geographic pattern to the supply of hospital facilities—beds, outpatient wards, specialised services—with some areas having surpluses and others deficits. Any planning must take these and future patterns of demand into account. Humane planning needs to predict the next areas of demand.

Geographers today know a lot about locating facilities to meet demands in a complex supply situation, but all the computer algorithms that could be used to solve such problems need the map predictions of where the mortally ill people will be. But the criteria for expanding old facilities and locating new ones are not simply economic. Health care facilities must be placed not only close to the people who need them, but also near those emotionally close to the patients. Humane planning demands nothing less.

And then we can educate, educate and educate, particularly young people on the threshold of adolescence, and too often with



GARY HALEY

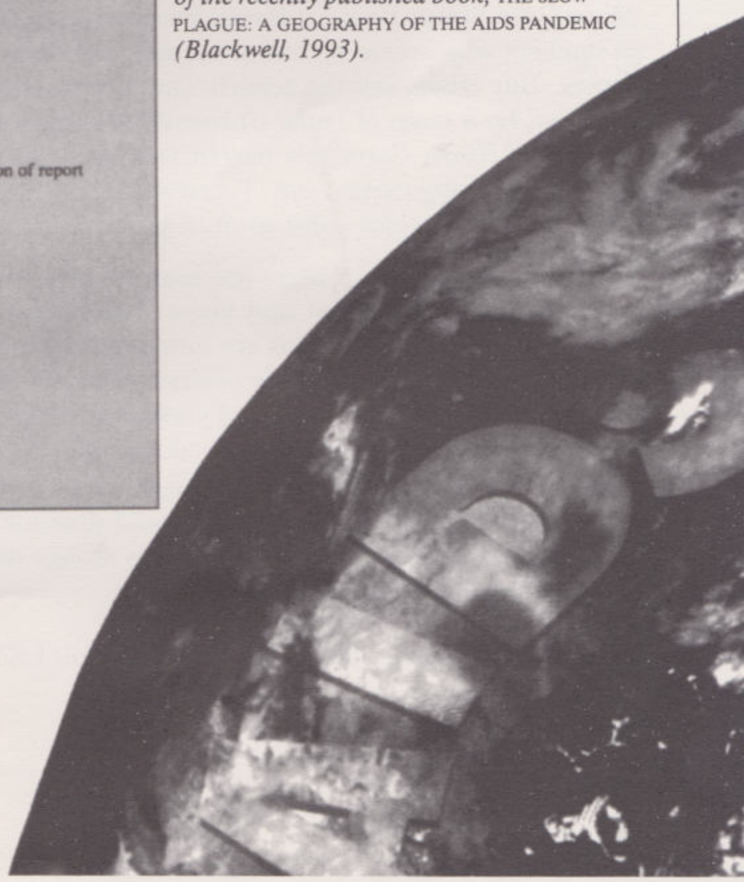


Mapping the geography of AIDS in the UK is still hampered by geographically ignorant reporting conventions. AIDS cases are reported by Regional Health Authority even though it is clear to all that the health planning requirements of, say, Edinburgh are very different to those of Skye

opportunities for drug experimentation. Medical educators talk about "cues to action", things that will take young people metaphorically by the scruffs of their necks and make them think about a deadly virus in quite personal terms. Animated map sequences for educational television, including predictions into the future, may constitute "cues to action", a seeing that is believing. One teenager viewing an animated map programme showing the diffusion of AIDS across Pennsylvania in our cartographic lab, said with awe in his voice: "Wow, man! I never realised it was that close!"

So if planning and education need map predictions, why have they not appeared on British television? Apart from the size and distribution of health areas, there appear to be two main reasons why Britain lingers so far behind. First, most modelling is caught up in purely temporal prediction, the sort that churns out numbers along the time horizon that no one can do anything with. What can you plan with a single number in Britain? Secondly, the medical profession, particularly the bureaucrats, have got to come down from the heights of Olympus, and join the mortals who inhabit the lower slopes. There they will find geographers who know what they are talking about. □

Peter Gould is Evan Pugh Professor of Geography at Penn State University and author of the recently published book, THE SLOW PLAGUE: A GEOGRAPHY OF THE AIDS PANDEMIC *(Blackwell, 1993).*



Source: Peter Gould, *Geographical*, Vol. LXVI, No. 1

Images of the world

Far from representing the world as it really is, Denis Wood says that maps are manipulated to bolster their creators' interests.

It's not just habit and sloth that predispose us to think of maps as pictures with all but unique claims to precision and truth. It's a whole way of thinking about pictures and communication that encourages it. This way of thinking, which we may call representation, insists not only on the independence of the world outside us, but also on the transparence of the gaze that gives us objective access to it. A map, therefore, as a more or less permanent and graphic instantiation of this 'objective' view, bears no responsibility for the existence of this world or for its reproduction.

This belief is wrong in almost every respect, as the most casual attention to the daily news attests. Rather than being a detached observer of a pre-existent world, the map is a committed participant carrying significant responsibility for bringing the world as we know it into being. Take the conflict in Bosnia, for instance (even to say 'Bosnia' is to acknowledge the power of the map). At one stage, diplomats were working secretly on a map that would grant 49 per cent of Bosnia to the Serbs and 51 per cent to the Muslim–Croat federation, according to one newspaper report. The Bosnian-Serb leader Radovan Karadzic responded to this by saying that: 'The contact group maps we heard about are humiliating.' In turn, this produced headlines like 'Bosnian Factions Ordered to Accept Map'. In none of this was there evidence of the map as an impartial tool that simply records the world it encounters.

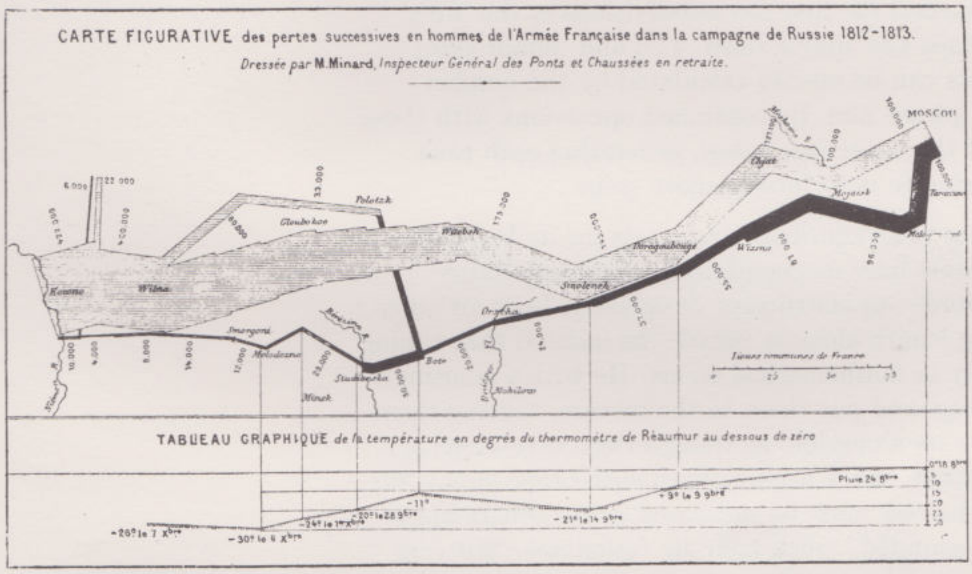
Other headlines, such as 'Mandela Weighs Request for Remapping' and 'Britain, Ireland Maps Way to Peace', highlight how maps are used to redefine the world rather than objectively represent it. One headline, 'Maps Rate Storm Risk for Beach Property', goes on to say: 'Duke researchers who access potential hazards say their work may help property buyers. But critics say the reports may be biased.' At issue were the maps produced by a team of Duke University scientists who were measuring stability of North Carolina's barrier islands. Even though the research had been scientifically carried out, because of their impact on property prices, the maps were at once considered to be biased.

But the truth is that all maps are biased—maps of rainfall, maps of nations in the Middle East and maps of spotted owl populations in the US Northwest—because all maps are forms of human discourse. And it is this discourse, and the mapmaker's position with the domain of this discourse, that calls the map into being and endows it with its subject and its form. To the extent that this discourse is dominated by power elites, the maps will embody their domination. Because of this, maps unavoidably become weapons in the perpetual battle for social dominion, though weapons disguised as impartial surveys of the way things really are. Those who fail to see through this fall victim to the map.

Source: Denis Wood (1994) *Geographical*, Vol. LXVI, No. 12

Narrative graphics of space and time

An especially effective device for enhancing the explanatory power of time-series displays is to add spatial dimensions to the design of the graphic, so that the data are moving over space (in two or three dimensions) as well as over time. [This] excellent space-time-story graphic illustrates how multivariate complexity can be subtly integrated into graphical architecture, integrated so gently and unobtrusively that viewers are hardly aware that they are looking into a world of [several] dimensions. Occasionally graphics are belligerently multivariate, advertising the technique rather than the data. But not [this one].



[This graphic] is the classic of Charles Joseph Minard (1781–1870), the French engineer, which shows the terrible fate of Napoleon's army in Russia. Described by E. J. Marey as seeming to defy the pen of the historian by its brutal eloquence, this combination of data map and time-series, drawn in 1861, portrays the devastating losses suffered in Napoleon's Russian campaign of 1812. Beginning at the left on the Polish-Russian border near the Niemen River, the thick band shows the size of the army (422,000 men) as it invaded Russia in June 1812. The width of the band indicates the size of the army at each place on the map. In September, the army reached Moscow, which was by then sacked and deserted, with 100,000 men. The path of Napoleon's retreat from Moscow is depicted by the darker, lower band, which is linked to a temperature scale and dates at the bottom of the chart. It was a bitterly cold winter, and many froze on the march out of Russia. As the graphic shows, the crossing of the Berezina River was a disaster, and the army finally struggled back into Poland with only 10,000 men remaining. Also shown are the movements of auxiliary troops, as they sought to protect the rear and the flank of the advancing army. Minard's graphic tells a rich, coherent story with its multivariate data, far more enlightening than just a single number bouncing along over time. *Six* variables are plotted: the size of the army, its location on a two-dimensional surface, direction of the army's movement, and temperature on various dates during the retreat from Moscow. It may well be the best statistical graphic ever drawn.

E. J. Marey (1885) *La Méthode Graphique*, Paris, p. 73. For more on Minard, see Arthur H. Robinson (1967) 'The Thematic Maps of Charles Joseph Minard,' *Imago Mundi*, 21, pp. 95–108.

Source: Edward Tufte (1983) *The Visual Display of Quantitative Information*

The Pythagorean Plato

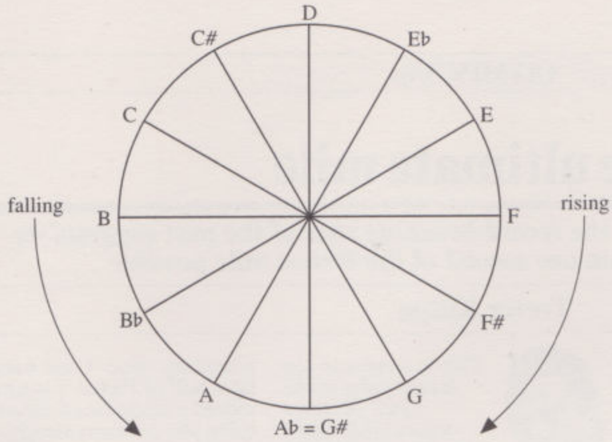
For possibly thousands of years before Plato, music provided a meaningful correspondence between number and tone in the readily observed and easily measured correlations between string lengths (on Hindu-Sumerian-Babylonian harps) and the tonal intervals they sound. Other factors being equal, halving a string length changes its pitch by an octave; the ratio $1:2$ thus corresponds to an acoustical experience—and a most important one, for every such division by 2 is perceived as a kind of 'cyclic identity' and is assigned the same letter name in modern notation. Subdivisions of this octave space can be defined as smaller ratios (i.e., between successively larger numbers). Although the ear cannot verify results with any accuracy beyond the first few subdivisions of the 'fifth' $2:3$, 'fourth' $3:4$, and perhaps the 'major third' $4:5$ and 'minor third' $5:6$, yet even micro-intervals can be readily calculated by the number theorist, to whatever limits please him, by continued operations with these same first six integers. And the tuner can follow, generating each tone from the last one, never daring to omit intermediate steps.

The very simplicity of tuning theory probably accounts for its being the first physical science to become fully mathematized. Although Plato obviously knew the monochord—an instrument designed to keep all other factors constant while string length alone is varied—he insisted that tuning theory be studied exclusively in mathematical terms. He writes amusingly of men who 'harass the strings and put them to the torture, racking them on pegs,' then setting their ears alongside 'as though they were hunting a voice from the neighbor's house,' while arguing with each other about 'the smallest interval by which the rest must be measured, while others insist that it is like those already sounded.' Such men, he complains, 'put ears before the intelligence.'

> They seek the numbers in these heard accords and don't rise to problems, to the consideration of which numbers are concordant and which not, and why in each case.
>
> (*Republic* 531a-c)

Plato's strong language requires that we too 'rise to problems' and think about numbers themselves and avoid deceiving ourselves that we can solve his problems simply by charting the tonal meanings of numbers along a monochord string. I will introduce schematic monochords in a few places, but mainly to render vivid to the reader the contradictions which arise when we ignore Plato's ridicule of that method.

Musicians who build and tune their own instruments know very well that the octave $1:2$ cannot be subdivided equally by the 'pure' musical ratios of rational numbers like those mentioned above. Today we arbitrarily subdivide the octave space into twelve equal parts so that a 'semitone' of $\frac{1}{12}$th of the octave has the numerical value $\sqrt[12]{2}$, and every larger interval is some multiple number of semitones, the smallest interval in the system. The integer ratios Plato knew are either slightly too large or too small to generate such a 'closed' system; they lead, in fact, toward the musical chaos of an infinite number of tones.



The equally tempered scale

An intimation of the problems which Plato dramatizes can be seen in the fact that powers of 2 (4, 8, etc.) defining octaves are *even* numbers which never coincide with powers of 3 (9, 27, 81, etc.) defining fifths and fourths by *odd* numbers, and neither of these series agrees with powers of 5 (25, 125, etc.) defining musical thirds. Tuning a 12-tone system is something of an art, then, for we must orient ourselves by the few 'pure' intervals which we can hear accurately and then slightly deform them—we call it 'tempering'—to ensure cyclic agreement.

Source: Ernest G. McClain (1978) *The Pythagorean Plato*

The ultimate mile

A mathematical analysis of the record-breaking runs of the past suggests we may already be within one second of the fastest mile possible

Trevor Kitson

HOW fast is it possible to run the mile? Until that famous day in May 1954 at the Iffley Road track, Oxford, many people considered that 4 minutes would never be beaten. It was the unsurmountable barrier. Nowadays of course this barrier is broken so easily and regularly that it hardly rates a mention in the sports pages. Twenty-three years after Roger Bannister's effort, the 3 min 50 s barrier was first overcome (by New Zealand's John Walker). And now we appear to be fast approaching the next "milestone" of 3 min 45 s.

How long can such improvements continue? Not forever—there must surely be an ultimate time, less than which it is humanly impossible to run the mile. Perhaps it is possible to predict this time from a study of the physiology and anatomy of legs and lungs, the biochemistry of muscle action, the psychology of the "will to win", and so on, but the most obvious indicator of things to come is to look at things past.

Fitting a curve to the fastest miles of the past 30 years suggests that Coe's record will stand for three more years

Peter Addis

Figure 1

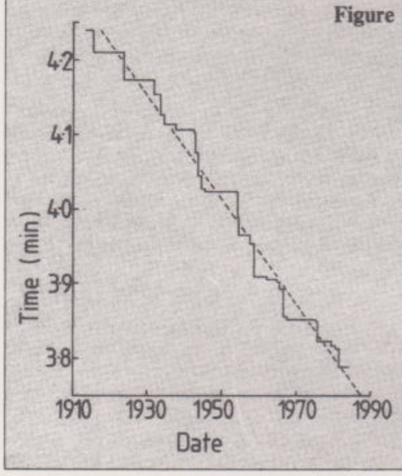
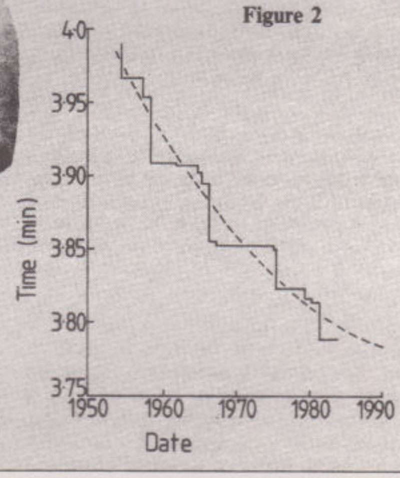


Figure 2



I plotted the record since 1913 against the date, and immediately several interesting points become apparent (Figure 1). For instance, the long horizontal steps show when the record was ripe for a change—periods such as 1945-1954, just before the 4 min barrier disappeared, and 1967-1975, when Jim Ryun of the US held the record for so long. Similarly the large steps downwards show up some of the more impressive bites which were taken out of the record. The largest single reduction was 2·7 s by Herb Elliott of Australia in 1958.

However, the most striking observation is the remarkable linearity of the graph. Over the past 70 years the world mile record has been eroded at a surprisingly steady rate with little sign at first glance that this change is tailing off (but see below). The dashed line is the best straight-line fit to the data, obtained by considering the value of the record as it stood on 1 January of each year from 1914 to 1984. This line (of correlation coefficient 0·991) may be represented by the equation:

$$t = 4{\cdot}358 - 0{\cdot}006933\,T$$

(where t = the time for the mile in minutes, and T = the date minus 1900). It predicts that the 3 min 45 s mark will fall in 1987 and that the mile will be run in 3 min 30 s by 2023. Indeed, projecting the line all the way to the axis allows us to foretell that on 1 August 2528 the mile will finally be run in no time at all, a feat which will presumably ruin athletics as a spectator sport.

To return to seriousness, it is of course ludicrous to consider that the straight line of Figure 1 will continue indefinitely into the future. Sooner or later it must tail off to a limiting value. Close examination of the later half of Figure 1 suggests that there is indeed a suspicion of curvature. In order to bring this out more clearly, the data for the record as it stood on 1 January and 1 June of each year since the 4 min barrier was broken (which seems as good an arbitrary starting point as any) were subjected to a general curve-fitting computer program written by my colleague Mike Hardman. The results are shown in Figure 2, in which the dashed curve is the best fit to the data and represents the equation:

$$t = 4{\cdot}777 - 0{\cdot}02039\,T + 0{\cdot}0001040\,T^2$$

The procedure of curve-fitting the results for the past 30 years, instead of the past 70, gives a very different prediction for the near future than discussed above. It suggests that Sebastian Coe's current world record of 3 min 47·33 s is likely to stand for about the next 3 years, and that thereafter the fall of the record will be minimal. The trend since 1954, leads to the conclusion that the "ultimate mile" will be run in 1998 in a time of 3 min 46·66 s, only marginally quicker than today's record.

There are of course many imponderables in trying to predict the future. Will drugs ever become accepted and legitimate? Will there be further significant improvements in track surfaces and shoes? How will training methods change as progress is made in research into physiology, biochemistry, nutrition and so on? At present, though, the only hard data on which to make a prediction are the previous records, and the outcome depends on how long a time period is used as a basis for calculation. From Figure 1 it would be completely unremarkable if the 3 min 45 s barrier were smashed before 1990, but Figure 2 shows, perhaps rather surprisingly, that this mark may never be beaten and that we may already be within one second of the ultimate mile. Time alone will tell if the trailing-off since 1954 will continue or if the record will lurch downwards again to regain or undercut the straight line of Figure 1. I look forward to the next few athletics seasons. □

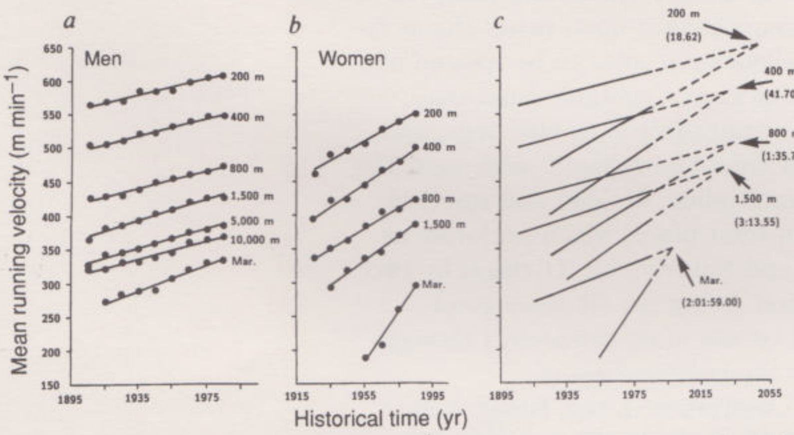
Dr Trevor Kitson lectures in the Department of Chemistry, Biochemistry and Biophysics at Massey University, New Zealand.

Source: *New Scientist*, 2 August 1984

Will women soon outrun men?

SIR — The mean running velocity ($\bar{V}$) is a crucial determinant of the metabolic demands imposed by competitive running events[1,2]. Its historical progression, therefore, is likely to be important in understanding the physiological determinants of the seemingly inexorable progression of record performances.

We therefore established $\bar{V}$ as a function of historical time (t) for the world records at all the standard Olympic events from the 200 m to the marathon (42,195 m) for men, decade-by-decade, throughout this century[3,4]. We were able to establish this relationship only for events up to 1,500 m since the 1920s for women; data were inadequate for the 5,000 and 10,000 m, although we judged there to be sufficient for the marathon.



World record progression, expressed as mean running velocity versus historical time, for men (*a*) and women (*b*), with best-fit linear regressions (solid lines) superimposed. In *c*, the regression lines for the common events for men and women (solid lines) are extrapolated (dashed lines) to their points of intersection; the predicted world record times at these intersection points are shown in parentheses (h:min:s)

In men, the progression of $\bar{V}$ appears to be a linear function of t, with slopes for the different events being remarkably similar (*a* in the figure) — in agreement with the results of Ryder *et al.*[5]. These ranged from 5.69 to 7.57 m min^{-1} decade, with no systematic variation with increasing race distance. The marathon slope, however, was appreciably greater (9.18 m min^{-1} decade).

For women, there were also no significant differences in the slopes among the different events up to the 1,500 m (*b* in the figure). The slope, however, was approximately double that for the men, ranging from 14.04 to 17.86 m min^{-1} decade. As for men, the rate at which $\bar{V}$ increased in the marathon was appreciably greater (37.75 m min^{-1} decade). Despite the potential pitfalls, we could not resist extrapolating these record progressions into the future.

Unless the progression rate of mens' records increases relative to that of women, then $\bar{V}$ for these events will be no different for men and women within the first half of the twenty-first century (*c* in the figure). Beyond that time, current progression rates imply superior performance by women. The projected intersection for the marathon is 1998.

The suggestion that women could, so soon, be running these races as fast as men seems improbable at first appearance. None of the current women's world record holders at these events could even meet the men's qualifying standard to compete in the 1992 Olympic games. However, it is the *rates* of improvement that are so strikingly different — the gap is progressively closing[6].

Although it is difficult to establish a precise metabolic energy equivalent of these rates of improvement, one may estimate from the data of Margaria *et al.*[7] and Wyndham *et al.*[8] that, for the events up to 10,000 m, the progression requires a rate of increase in O_2 consumption of about 10 ml min^{-1} yr for men and more than double that for women.

It is unlikely that we will learn when, and how rapidly, the current high rate of improvement was established, owing to the lack of reliable times over reliable distances in the past. Some world records are available however, as far back as the 1860s (ref. 4); they are consistent with the values 'expected' from the current progression rates. Whether the world record progression rate will begin to slow, either relatively abruptly or more progressively, will only become apparent in the future.

In any event these results pose four challenging questions to physiologists. Why is: the world record progression in the various events so linear over an interval of approximately a century; the slope of the record progression so similar from the sprints to the 10,000 m; the record progression in the marathon appreciably greater; and the record progressions for women increasing at such a rapid rate relative to men?

BRIAN J. WHIPP
SUSAN A. WARD

Departments of Physiology and Anesthesiology,
School of Medicine,
University of California,
Los Angeles, California 90024, USA

1. Lloyd, B. B. *Circ. Res.* **XX** & **XXI** (suppl. 1), I218–226 (1967).
2. Di Prampero, P. E. *Rivista di Cultura Sportiva* **3**, 3–7 (1984).
3. Matthews, P. *Track and Field Athletics: The Records* (Guinness, Enfield, 1986).
4. *Progression of World's Best Performances and Official I.A.A.F. World Records* (International Athletic Foundation, Monaco, 1987).
5. Ryder, H. W., Carr, H. J. & Herget, P. *Sci. Am.* **234**, 6 109–119 (1976).
6. Dyer, K. J. *J. Biosocial Sci.* **9**, 325–338 (1977).
7. Margaria, R., Cerretelli, P., Aghemo, P. & Sassi, G. *J. appl. Physiol.* **18**, 367–371, (1963).
8. Wyndham, C. H., Strydom, N. B., Van Rensburg, A. J. & Benade, A. J. S. *S. Afr. Med. J.* **43**, 996–1002 (1969).

Source: *Nature*, 2 January 1992

Logarithms

John Napier's preface to A Description of the Admirable Table of Logarithms

Seeing there is nothing (right well beloved students in the Mathematics) that is so troublesome to Mathematicall practise, nor that doth more molest and hinder Calculators, than the Multiplications, Divisions, square and cubical Extractions of great numbers, which besides the tedious experience of time, are for the most part subject to many slippery errors. I began therefore to consider in my minde, by what certaine and ready Art I might remove those hindrances. And having thought upon many things to this purpose, I found at length some excellent brief rules to be treated of (perhaps) thereafter. But amongst all, none more profitable than this, which together with the hard and tedious Multiplications, Divisions, and Extractions of rootes, doth also cast away from the worke it selfe, even the very numbers themselves that are to be multiplied, divided and resolved into rootes, and putteth other numbers in their place, which performe as much as they can do, onely by Addition and Subtraction, Division by two or Division by three; which secret invention, being (as all other good things are) so much the better as it shall be the more common; I thought good heretofore to set forth in Latine for the publique use of Mathematicians. But now some of our Countrymen in this Island well affected to these studies, and the more publique good, procured a most learned Mathematician to translate the same into our vulgar English tongue, who after he had finished it sent the coppy of it to me, to be seene and considered on by myself. I having most willingly and gladly done the same, finde it to be most exact and precisely conformable to my minde and the originall. Therefore it may please you who are inclined to these studies, to receive it from me and the Translator, with as much good will as we recommend it unto you. Fare yee well.

William Lilly on the meeting of Napier and Briggs

I will acquaint you with one memorable Story related unto me by Mr John Marr, an excellent Mathematician and Geometrician, whom I conceive you remember; he was Servant to King James and Charles the First.

At first, when the Lord Napier, or Marchiston, made publick his *Logarithms*, Mr Briggs, then Reader of the Astronomy lecture at Gresham College in London, was so surprized with Admiration of them, that he could have no Quietness in himself, untill he had seen that noble Person the Lord Marchiston, whose only Invention they were; he acquaints John Marr herewith, who went into Scotland before Mr Briggs, purposely to be there when these Two so learned Persons should meet: Mr Briggs appoints a certain Day when to meet at Edinborough, but failing thereof: the Lord Napier was doubtful he would not come: It happened one Day as John Marr and the Lord Napier were speaking of Mr Briggs: 'Ah, John (saith Marchiston), Mr Briggs will not now come': at the very one knocks at the gate; John Marr hasted down, and it proved Mr Briggs, to his great Contentment: he brings Mr Briggs up into my Lord's chamber, where almost one quarter of an hour was spent, each beholding other almost with Admiration before one word was spoke, at last Mr Briggs began.

'My lord, I have undertaken this long Journey purposely to see your Person, and to know by what Engine of Wit or Ingenuity you came first to

think of this most excellent Help unto Astronomy, *viz.*, the *Logarithms*; but, my Lord, being by you found out, I wonder nobody else found it out before, when now known it is so easy.' He was nobly entertained by the Lord Napier, and every Summer after that, during the Lord's being alive, this venerable Man, Mr Briggs, went purposely into Scotland to visit him.

John Keil on the use of logarithms

The Mathematicks formerly received considerable Advantages; first, by the Introduction of the Indian Characters, and afterwards by the Invention of Decimal Fractions; yet has it since reaped at least as much from the Invention of Logarithms, as from both the other two. The Use of these, every one knows, is of the greatest Extent, and runs through all Parts of Mathematicks. By their Means it is that Numbers almost infinite, and such as are otherwise impracticable, are managed with Ease and Expedition. By their assistance the Mariner steers his Vessel, the Geometrician investigates the Nature of the higher Curves, the Astronomer determines the Places of the Stars, the Philosopher accounts for other Phenomena of Nature; and lastly, the Usurer computes the Interest of his Money.

Source: John Fauvel and Jeremy Gray (eds) (1987) *The History of Mathematics: a reader*, Macmillan Education/The Open University

* * *

Do you need to know how it works? by Costel Harnasz

What can a dispute that occurred in seventeenth-century England over who invented the circular slide rule offer students and teachers today? The slide rule has all but disappeared—it is difficult to come across one these days. More often than not they have become something like a family heirloom, or an object of curiosity, because the knowledge of how to operate it has, like grandfather, passed away.

However, they once had their heyday, and it is really quite astonishing and interesting to reflect on how quickly the slide rule disappeared from the scene. Within the space of a few years they had been ousted by the arrival of the electronic calculator. Astonishing, because they had been continually refined and modified and in use for so long. Interesting because some of the arguments that arose in connection with the silicon-chip-based pretender echoed those being voiced some 350 years before.

But first, some background. What is a slide rule? It is a pair of rules, each with its own scale, fitted in such a way that the scale of one slides along the scale of the other. In its simplest form it can be used for adding and subtracting, as shown in Figure 1. In fact, two ordinary rulers can be used to perform the calculation in this way. Suppose we wished to add together 5 and 2.5. Scale A is moved until its zero is opposite 2.5 on scale B. We then look along scale A until we come to 5 and find the number that lies opposite on scale B, in this case 7.5. Of course, we could add any number to 2.5 in this way, and perhaps invent a cursor to move along the scales to make the reading easier. The inverse operation, subtraction, is the addition operation performed in reverse.

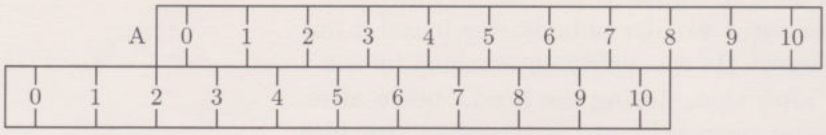


Figure 1

Now normally discussion about slide rules brings the operations of multiplication and division into mind. We do not usually need a device or instrument of this sort to do simple additions and subtractions—although it can have its use as a teaching aid if it is thought of as two moveable number lines. But I shall leave that to be explored in the classroom.

Now it may come as a surprise that multiplication can be carried out using a slide rule. Or at least there is usually acknowledged to be some element of mystery about this. But take Figure 2. Here are two scales, maybe made on paper or thin card, with the marks on each of them placed at intervals of equal length. To multiply 10 by 1 000 you set them up as shown in Figure 3, reading the answer 10 000 on the bottom scale, opposite 1 000 of the upper one, which has had its '1' placed adjacent to the '10' mark as shown.

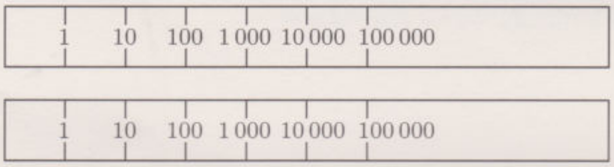


Figure 2

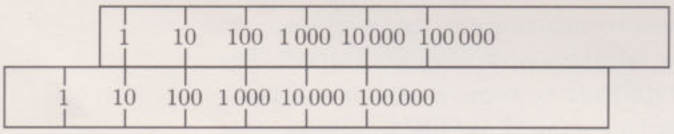


Figure 3

This is fine if you want to confine yourself to multiplying and dividing these powers of ten, but what is needed is a way of marking the numbers between 1 and 10, 10 and 100, and so on. And in fact, this is the basis of an investigation in *Starting Points* by Banwell *et al.* (1986) and from *The School Mathematics Project, Book 2* (SMP, 1969) from which the following quotation is taken.

> Advances in scientific achievement have always resulted in a demand for ways of speeding up the arithmetical work associated with problems. Galileo's invention of the telescope, for example, produced more accurate measurements of the large distances involved in astronomy. These more accurate measurements resulted in more arithmetical work; consequently, it was no coincidence that, in the early seventeenth century, John Napier pioneered a new technique for calculation at the same time as Galileo was observing the movements of the moon and planets.
>
> We shall approach the problem in the way that Napier did.
>
> (SMP, 1969: 176)

You may have heard of or seen Napier's bones (Figure 4), maybe it is one of those phrases that just sticks in the mind and comes up at intervals in quizzes and bad puns. Napier's bones are in fact one of several

computational aids which he invented. They consisted of the columns of a multiplication table inscribed onto rods. These are then aligned, choosing th appropriate ones for the calculation being performed, in such a way that the 'partial products' are readily obtained and displayed in a fashion which makes adding them up straightforward. In a sense it is a little like a partly mechanised form of long multiplication.

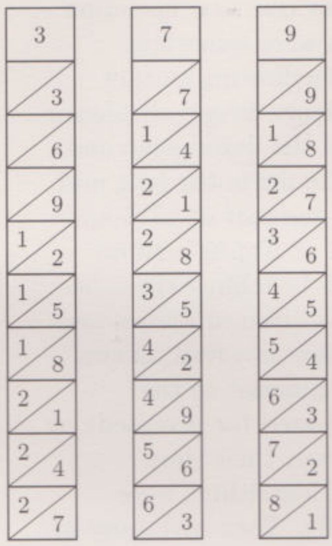


Figure 4 Example of Napier's rods (or bones) set up to show multiplication by 379

But Napier achieved greatest fame for his invention of logarithms. Looking back at the scales for multiplying powers of 10 it can been seen that the task of multiplying two numbers can be changed to the simpler method of addition—in this case it turns out to be adding the number of zeros. Ten does not have to be adhered to as a base; for example, if we work with another geometric progression as illustrated in Figure 5, i.e. where the common ratio is now 2, it can readily be seen that if any two numbers in the upper sequence, e.g. 4 and 8, are multiplied, their product (in this case, 32) is to be found above the sum of 2 and 3 in the bottom line. When the sequence is written as in Figure 6 it can be seen that it works because

$$4 \times 8 = 2^2 \times 2^3 = 2^5 = 32,$$

i.e. powers of two are added and this is the basis of his discovery. Napier coined the word *logarithm* from Greek words meaning 'ratio-number'.

$$\begin{array}{ccccccccccc} \cdots & \frac{1}{4} & \frac{1}{2} & 1 & 2 & 4 & 8 & 16 & 32 & 128 & \cdots \\ & -2 & -1 & 0 & 1 & 2 & 3 & 4 & 5 & 6 & \end{array}$$

Figure 5

$$\begin{array}{ccccccccccc} \cdots & \frac{1}{4} & \frac{1}{2} & 1 & 2 & 4 & 8 & 16 & 32 & 128 & \cdots \\ & 2^{-2} & 2^{-1} & 2^0 & 2^1 & 2^2 & 2^3 & 2^4 & 2^5 & 2^6 & \end{array}$$

Figure 6

Although I have used pictures and scales to aid an understanding of logarithms, and have discussed slide rules from the beginning of the chapter, the invention of the slide rule actually followed that of the logarithm. A number of school texts up to the 1970s which dealt with this topic also precede their description of logarithms by a discussion of the slide rule. Napier's description of logarithms was published in 1614, in Latin, with the title *Mirifici Logarithmorum Canonis Descriptio.* This is

interesting, for until that time he had becomely widely known for having written (in English) a religious Protestant treatise that appeared in 1594, and this suggests that he was trying to reach a more learned and possibly international audience with his mathematical work. Another date for the time line is that Elizabeth I had died in the intervening period, in 1603, ushering in the Stuart era.

What happened next is history! The time was ripe for this new invention to sweep across Europe. It was clear that logarithms were something different and that they held the promise for greatly facilitating certain calculations. This was no more keenly felt than by Henry Briggs, Professor of Geometry at Gresham College in London. He read the description and decided that he just had to meet Napier in person. He made the long and difficult journey to Napier's home in Scotland in the summer of 1615 and an account of their first meeting upon Briggs' arrival at Napier's house 'where almost one quarter of an hour was spent, each beholding the other almost with admiration before one word was spoke' is given in Fauvel and Gray (1987). This illustrates the very human dimension involved, of two people collaborating closely together. How this is in contrast to the commonly held notion of mathematics as being discovered (or invented) by the mathematician working on his or her own! However, their time together was fruitful, the practical working details of logarithms were 'debugged' and soon knowledge of them spread rapidly. They had a use in navigation as the world was being opened up, and on the continent, Kepler, who was reshaping the universe, was so moved when he read a copy of Napier's book that he dedicated his next to work to him.

A turning point was reached in which cultures clashed. An old order was changing. Michael Mästlin, Kepler's old astronomy teacher, was rather distrustful of logarithms. He made two points: first, that mathematicians should not be using these new tables if they did not know or understand how they had been constructed, because if the calculations worked in specific cases there was no certainty that they would always do so. Further, in a delicious turn of phrase he told Kepler, 'it is not seemly for any professor of mathematics to be childishly pleased about any shortening of the calculations' (Open University, 1987). We can still hear this today, in the form of children being unable to do 'long' multiplication or division. I am not sure if the abacus engendered such sentiments, but a debate was started which continued to rumble through the centuries to surface a couple of decades ago when logarithm tables were being displaced by calculators. This tension between mathematical education practice and technical innovation can even occur within advances in the same technology. Witness the reconsideration of what 'curve sketching' means with the availability of cheap and powerful programmable graphics calculators. More recently, the same sort of reaction has occurred with the use of computers in mathematics, making us queasy about what can constitute a proof, as happened when the four-colour map problem recently finally surrendered to programming power.

However, returning to the early seventeenth century, a key advocate and proponent of logarithms at the time was Edmund Gunter, who in addition to lecturing at Gresham College also invented and modified a number of mathematical instruments of the period. He was the first to mark out the new logarithms onto a ruler—using a pair of dividers to transfer the distances along the ruler and so make handy, usable multiplications. This was the first step towards the slide rule.

However, another mathematician of the time, William Oughtred, found this new logarithmic rule rather awkward to use, and he took two such scales and put them next to each other so that they could slide and perform the kind of operation observed in Figures 2–3. So was born the slide rule.

But soon came the dispute; for Oughtred had also devised another form of slide rule in which circles of different sizes were each marked with a logarithmic scale joined at the centre so that they could rotate. He called them his 'circles of proportion'. Again, multiplication was carried out by adding a length on one scale to a length marked on the other. But it was a former pupil of his, Richard Delamain, who had a description of a similar instrument, published in 1630. An argument broke out between them over who had been the first to invent the circular slide rule. Who were these men that had this unseemly argument? After all, it is not the sort of behaviour that is usually associated with mathematicians! Oughtred was a clergyman of private means, and a key figure of the time. He had written a book entitled *Clavis Mathematicae*, or *Key to Mathematics*, which helped spread the notion or working with algebraic symbols. It was one of the books that Isaac Newton used when an undergraduate at Cambridge. He was also an influential teacher whom many students sought, for instance Christopher Wren had been among his pupils. Less is known about the younger man. Delamain had started off as a joiner but by attending the Gresham lectures and mixing with learned men he acquired enough knowledge to become a 'teacher of practical mathematics'. He eventually became wealthy and successful from the sales of his mathematical instruments and handbooks.

As a result of the rivalry surrounding this priority claim there is a body of writing that gives an insight into the atmosphere of the mathematical community of the day and of the changing relationship between mathematics and society, and also of the issues of the teaching of mathematics. Oughtred took the view that one should understand the theory behind the mathematical instruments that were being used.

> The true way of Art is not by instruments but by demonstration. The use of instruments is indeed excellent if a man be an artist but contemptible being opposed to Art.
>
> (Cajori, 1916: 88)

Delamain took the opposite view, stating

> for no-one to know the use of a Mathematical Instrument, except he knows the cause of its operation, is somewhat too strict, which would keep many from affecting the Art, because they see nothing but obscure propositions, and perplex and intricated demonstrations before their eyes.
>
> (ibid.: 90)

So here are two clearly contrasting views about the use of mathematical instruments. Oughtred favouring theoretical understanding and Delamain saying it was not necessary.

Oughtred said that certain teachers (with perhaps Delamain in mind) made their pupils 'only doers of tricks and as it were, jugglers' (Cajori 1916: 88). This comment too, perhaps, has a resonance today when pupils are using calculators and they display an answer that may be hopelessly out, but by accepting what the display says draw the thought from

teachers that 'they don't really understand what they are doing'. Delamain was quick to defend himself from these words of criticism as

> they are touching, and pernitious, by too much derogating from many, and glancing upon many noble personages, with too grosse, if not too base an attribute, in terming them doers of tricks, as it were to juggle. (Cajori, 1916: 89)

You can imagine what the young wealthy scholars were asking of Delamain: 'look, I'm not interested in how it works, just tell me what to do.'

Perhaps Oughtred was a little put out by Delamain's success. There was a commercial side to this, too. The technology for making the instruments of the time, sundials, quadrants and so on, was being used in London, and there was money to be made.

In fact, at the heart of the matter is the way they differed in their attitude to the teaching of mathematics. Delamain said that pupils could start with the instruments straight away even if they did not understand the reasoning behind them. Besides, gentlemen did not want to learn very much mathematics, just what was necessary for their work, being the management of estates, the navy, trade or administration, all of which were coming to require some skill in the subject. Oughtred, however, emphasised the need for rigorous thinking.

There remains the paradox that Oughtred, in his student days and subsequently, had invented sundials, planispheres and various types of slide rules, so why did he discourage the use of instruments in teaching mathematics to beginners? On the face of it, his espoused pedagogy goes against the story of his own intellectual development. But then, he had a passion for the subject while a student in Cambridge, at a time when it had no particular emphasis. Therefore he would have really tried to work things out for himself, and besides, it is known that he took a delight in assisting his fellow students.

Meanwhile, the realm of England was undergoing upheaval. Society was changing and there was an attack on established forms of learning. Mathematics could not continue to exist as a pure body of knowledge, but was becoming useful to society in a way that hadn't been seen before. The general instability led to the civil war which was about to take place, during which Delamain perished and the first King Charles was beheaded. Oughtred himself managed to escape sequestration and died fifteen years later, in 1660, near Guildford where he had been this clergyman with an avid interest in mathematics.

So, at the end of the day, who had the right approach, Oughtred or Delamain? Perhaps they can stand as metaphors, not just for the issues concerning the employment of new technologies today, but in the current 'return to traditional methods of teaching' debate. Isaac Newton made sundials, windmills and a clock in his childhood. Would we stop or encourage such activities?

Perhaps then we can consider a balance between the two. Of developing that kind of awareness when the time is right to be concrete and practical but being sensitive to the moment when it is appropriate and fruitful to enter an abstract realm, and develop insight.

References

Banwell, C. S., Saunders, K. D. and Tahta, D. S. (1986) *Starting Points*, Diss: Tarquin Publications.

Cajori, F. (1916) *William Oughtred—A Great Seventeenth Century Teacher of Mathematics*, Chicago: Open Court.

Fauvel, J. and Gray, J. (eds) (1987) *The History of Mathematics: a Reader*, London: Macmillan.

Open University (1987) *The Renaissance of Mathematical Sciences in Britain* (*Unit 6* of the Open University *History of Mathematics* course), Milton Keynes: Open University Press.

SMP (1969) *The School Mathematics Project, Book 2*, Cambridge: Cambridge University Press.

Source: Michelle Selinger (ed.) (1994) *Teaching Mathematics*, Routledge

Sorry, no King Kongs

This piece is concerned with the physical effects of uniform scaling.

Example: What about a 10-foot cube?

If we scale the original cube of steel up to a cube 10 feet on a side, then the dimensions are

$$10\,\text{ft} \times 10\,\text{ft} \times 10\,\text{ft}$$

The total volume is

$$\begin{aligned} V &= \text{length} \times \text{width} \times \text{height} \\ &= 10\,\text{ft} \times 10\,\text{ft} \times 10\,\text{ft} \\ &= 1000\,\text{ft} \times 1\,\text{ft} \times 1\,\text{ft} = 1000\,\text{ft}^3 \\ &= 1000\,\text{cu ft} \end{aligned}$$

The weight of the cube is

$$\begin{aligned} W &= V \times \text{density} \\ &= 1000\,\text{cu ft} \times 500\,\text{lb/cu ft} \\ &= 500,000\,\text{lb} \end{aligned}$$

The area of the bottom face is

$$\begin{aligned} A &= \text{length} \times \text{width} \\ &= 10\,\text{ft} \times 10\,\text{ft} \\ &= 100\,\text{ft} \times 1\,\text{ft} \\ &= 100\,\text{ft}^2 = 100\,\text{sq ft} \end{aligned}$$

The pressure on the bottom face is

$$P = \frac{W}{A} = \frac{500,000\,\text{lb}}{100\,\text{sq ft}} = 5000\,\text{lb/sq ft}$$

This is 10 times—not '10 times more than'—the pressure on the bottom face of the original 1-foot cube.

At some scale factor the pressure on the bottom face will exceed the steel's ability to withstand that pressure—and the steel will deform under its own weight. That point for steel is reached for a cube about 3 miles on a side—the pressure exerted by the cube's weight exceeds the resistance to crushing (ability to withstand pressure, or **yield strength**) of steel, which is about 7.5 million lb/sq ft. Since a mile is 5280 feet, a 3-mile-long cube of steel would be more than 15,000 times as long as the original 1-foot cube; that is, the scaling factor is more than 15,000. The pressure on the bottom face of the cube would therefore be more than 15,000 times as much as for the 1-foot cube, or $15,000 \times 500\,\text{lb/sq ft} = 7.5$ million lb/sq ft.

...

Unfortunately, the resistance of bone to crushing is not nearly as great as that of steel. This fact helps to explain why there couldn't be any King Kongs (unless they were made of steel!). A creature scaled up by a factor of 20 would weigh $20^3 = 8000$ times as much. Though the weight increases with the cube of the scaling factor, the ability to support the weight—as measured by the cross-sectional area of the bones, like the area of the bottom face of the cube—increases only with the square of the scaling factor.

These simple consequences of the geometry of scaling apply to other objects, natural and artificial, not only to supermonsters. For example, we can estimate how high the tallest mountains could be. Three hundred and fifty years ago, Galileo was able to give an accurate estimate of how high the tallest trees could be . . . .

Example: How high can a mountain be?

Real mountains, of course, aren't made out of steel, and they don't come in the shape of a cube. Mountains differ from one to another in composition and shape, and some assumptions about those features will be necessary in order to do any calculating. We want to make our assumptions as realistic as we can and still be able to calculate easily an estimate of how high a mountain can be. In effect, we build a simple mathematical model of a mountain.

Let's suppose that the mountain is made entirely of granite, a common material in many mountains, and we assume that the granite has uniform density. Relevant facts about granite are that it weighs 165 lb/cu ft and it has a yield strength of about 30 million lb/sq ft.

In the interests of both realism and simplicity, we assume that our model mountain is in the shape of a cone whose width at the base is the same as its height. Let's model Mount Everest: the tallest earth mountain, it is about 6 miles high. The base, then, is a circle with a distance across (or diameter) of 6 miles. The radius of the circle is half the diameter, so our model Everest has a radius of 3 miles measured at the base (Figure 7). Since we are taking such a round number for the height of Everest, we will record as significant only the first two digits of the results of our calculations.

What does our model Everest weigh? The relevant formula is

$$\text{Weight} = \text{density} \times \text{volume}$$

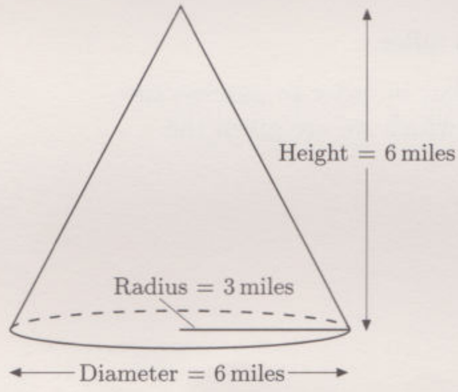


Figure 7 Model of Mt. Everest as a cone of granite

We already know the density of granite (165 lb/cu ft), so to find the weight we are going to need to know how to calculate the volume of a cone. The formula is

$$\text{Volume} = \pi \times (\text{radius})^2 \times \frac{\text{height}}{3}$$

For our Everest, the radius is 3 miles and the height is 6 miles; π (pi) is about 3.14. Using those values in the formula, we find that our model Everest has a volume of about 57 cubic miles.

To find the weight of 57 cubic miles of granite, we need to do some conversion of units, since the density is given in pounds per cubic foot. Let's convert to units of feet:

$$\begin{aligned}
1\,\text{cu mi} &= 1\,\text{mi} \times 1\,\text{mi} \times 1\,\text{mi} \\
&= 5280\,\text{ft} \times 5280\,\text{ft} \times 5280\,\text{ft} \\
&= 1.5 \times 10^{11}\,\text{ft} \times 1\,\text{ft} \times 1\,\text{ft, approximately} \\
&= 1.5 \times 10^{11}\,\text{cu ft, approximately}
\end{aligned}$$

and

$$\begin{aligned}
57\,\text{cu mi} &= 57 \times 1\,\text{cu mi} \\
&= 57 \times 1.5 \times 10^{11}\,\text{cu ft, approximately} \\
&= 8.6 \times 10^{12}\,\text{cu ft, approximately}
\end{aligned}$$

So we have

$$\begin{aligned}
&\text{Weight of mountain} \\
&\quad = 165\,\text{lb/cu ft} \times 8.6 \times 10^{12}\,\text{cu ft} \\
&\quad = 1.4 \times 10^{15}\,\text{lb} \\
&\quad = 1.4\ \text{quadrillion lb}
\end{aligned}$$

Now that we know the weight of the mountain, we want to find out what the pressure is on the base of the cone and compare that with the yield strength of granite. (Everest is standing, so if our model is any good, that pressure will be below the yield strength.) Physics tells us that the weight of the mountain is spread evenly over the base of the cone (we are oversimplifying the geology underlying mountains). Since

$$\text{Pressure} = \frac{\text{weight}}{\text{area}}$$

we need to calculate the area of the base of the cone. The shape is a circle, and the familiar formula

$$\text{Area} = \pi \times (\text{radius})^2$$

gives an area of 28 square miles for a radius of 3 miles.

Once again, we will need to convert to units of feet in order to express the pressure in pounds per square foot, the units in which we are given the yield strength. We get

$$\begin{aligned}
\text{Area} &= 28\,\text{sq mi} \\
&= 28 \times 1\,\text{mi} \times 1\,\text{mi} \\
&= 28 \times 5280\,\text{ft} \times 5280\,\text{ft} \\
&= 8 \times 10^{8}\,\text{sq ft, approximately}
\end{aligned}$$

Then

$$\begin{aligned}
\text{Pressure} &= \frac{\text{weight}}{\text{area}} \\
&= \frac{1.4 \times 10^{15}\,\text{lb}}{8 \times 10^{8}\,\text{sq ft}} \\
&= 1.7 \times 10^{6}\,\text{lb/sq ft} \\
&= 1.7\ \text{million lb/sq ft}
\end{aligned}$$

This number is safely below the yield strength of granite, 30 million pounds per square foot.

For a mountain to come close to the limitation of the yield strength of granite, it would have to be about 18 (= 30 million/1.7 million) times as

high as Everest, or about 100 miles high. From other physical considerations we can determine that the maximum possible height of a mountain on earth would be closer to 15 miles. That no present mountains are that high is a consequence of the earth's high amount of volcanic activity and structural deformation of the earth's crust.

What about mountains made of other materials—glass, ice, wood, old cars? They couldn't be nearly as high; the pressure would cause glass to flow, ice to melt, and old cars to compact. What about mountains on another plant? Their potential height also depends on the gravity of the plant.

...

A large change in scale forces a change in either materials or form. A major manifestation of the scaling problem is the tension between weight and the need to support it. For example, a real building or machine must differ from a scale model; the balsa wood or plastic of the model would never be strong enough to use for the real thing, and the materials in the scaled-up version must be aluminium, steel, or reinforced concrete. So one way to compensate for the problem of scale is to use stronger materials in the scaled-up object .... .

The other way to compensate is to redesign the object so that its weight is better distributed. Let's go back to our original cube. It supports all its weight on its bottom face. In the version scaled up by a factor of 3, each small cube of the bottom layer has a bottom face that is supporting that cube's weight plus the weight of the other two cubes piled on top of it.

Now, let's redesign the scaled-up cube, concentrating for simplicity only on the front face, with its nine small cubes. We take the three cubes on top and move them to the bottom, alongside the three already there. We take the three cubes on the second level, cut each in half, and put a half cube over each of the six ground-level cubes (see Figure 8). We have the same volume and weight that we started with, but now there is less pressure on the bottom face of each small cube. Of course, our new design is not geometrically similar to the object we started with—it's not longer a cube. By changing the proportions, we have given up the precise scaling of geometrical similarity, but we have managed to compensate for the scaling problem.

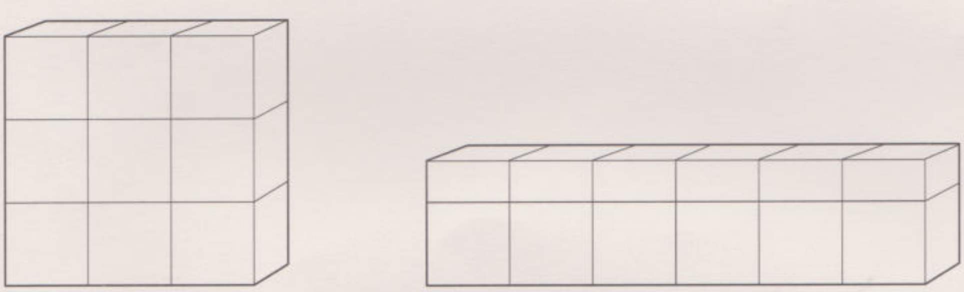


Figure 8 Nine small cubes rearranged to support greater weight

We observe in nature both strategies for adaptation to scaling: change of materials and change of form. Smaller animals generally do not have bony internal skeletons; larger animals generally do. Those animals made of similar materials but differing greatly in size, such as a mouse and an elephant, will most certainly differ in shape. If a mouse were scaled up to the size of an elephant, its legs could no longer support it. It would need the disproportionately thicker legs of the elephant, as well as the elephant's thick hide to contain its tissue.

Some dinosaurs, like *Supersaurus* (which weighed 30 tons, as much as a tank), had special adaptations to lighten their weight, such as hollow bones, just as some birds have. (Hollow bones also turn out to be stronger, a paradox that Galileo analyzed. Of two bones of the same weight and length, the hollow one will be wider across at its midpoint, because of the air it contains; and the greater the width, the greater the resistance to fracture.)

Source: L. A. Steen *et al.* (1991) *For All Practical Purposes*

Motion control of MSVF lift drive

Objective

To describe how the variable frequency drive operates and to show the range covered by the Medium Speed Variable Frequency Drive (MSVF).

Description

In VF systems, the voltage and frequency of the AC power supplied to a lift drive is varied, using pulse width modulation. The voltage and frequency parameters of the supply to the drive are adjusted in real-time to ensure that the optimum torque is obtained from the drive motor at all points in the designed speed profiles. This gives total control of car motion, regardless of load, and maximises drive performance and efficiency....

Closed loop *speed and distance* control ensures consistent speed profiles with optimum ride quality, regardless of load. Accurate speed and position is achieved via a digital encoder mounted on the motor's shaft.

Prior to the brake lifting, the motor is pre-torqued to prevent roll back. This gives a smooth start to the lift journey. The system will determine the distance the lift has to travel and select the fastest flight profile possible. The drive will always try to reach its destination as fast as possible.

The MSVF drive packages covers all lift speeds of 1.0, 1.6, 2.0 and 2.5 m/s.

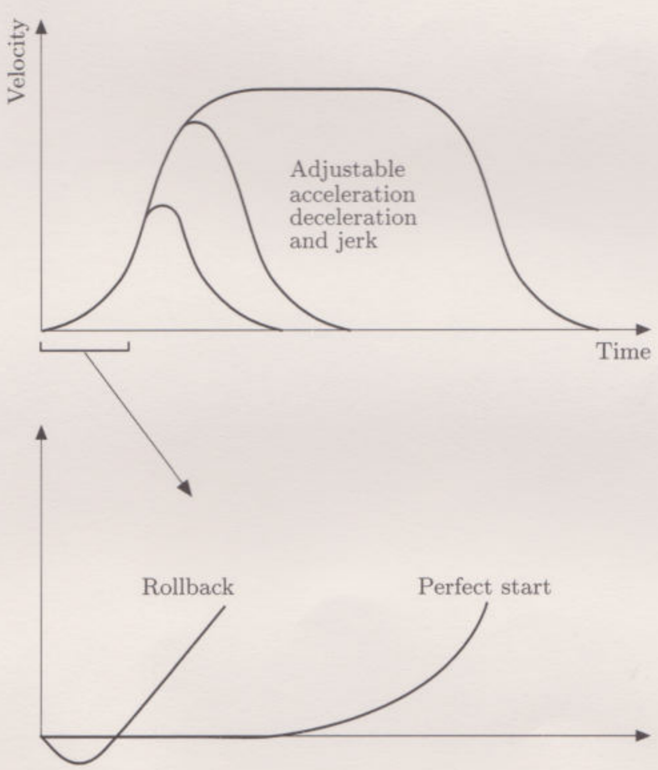


Figure 9 MSVF velocity profiles

Customer benefit

Improved ride quality and levelling accuracy because the system has closed loop control and *speed and position* which are constantly monitored and adjusted depending on the distance from the destination landing.

Fully adjustable flight profiles with fast short floor capability which helps to reduce floor to floor times even more.

Practically no wear on the machine brake linings because break is applied only when the lift car has stopped compared to 2-speed AC. Motor pre-torqueing avoids car roll-back and ensures a smooth start before the brake is released compared to Servo and LSVF.

Reduced costs to the building owner because of improved power factor and reduced energy consumption compared to 2-speed and Servo and therefore more efficient use of incoming power.

Source: OTIS technical documents (1993)

Bluebird correspondence

In September and October 1956, a lively debate on the different ways of calculating 'average speed' was conducted in *The Times*. Extracts are reproduced below.

BLUEBIRD'S SPEED

TO THE EDITOR OF THE TIMES

Sir,—The trouble which Mr Donald Campbell experienced in his second run in Bluebird on September 19 has led to an anomalous result. The regulation which requires two runs over the measured kilometre in opposite directions is intended to allow for the effect of such factors as current and wind. The arithmetic mean of the two speeds in opposite directions gives the true speed of the boat, provided that this true speed is constant and that current and wind are also constant. When, however, mechanical trouble causes the boat to travel at a much reduced speed on one run, the arithmetic mean gives a value which is much higher than the actual average speed.

In Mr Campbell's first run he achieved a speed of 286.78 m.p.h., which corresponds to a time of 7.8005 sec. for the measured kilometre; in his second run his speed was 164.48 m.p.h., corresponding to a time of 13.601 sec. Hence his average speed which is given by the total distance of two kilometres divided by the total time of 21.4015 sec., is 209.46 m.p.h. This is the harmonic mean and it is lower than the previous record. As it is impossible in such a case as this to make allowance for the wind or current, this harmonic mean may not be the correct value, but it is very much nearer to the true average speed of the boat than is the arithmetic mean of 225.63 m.p.h. Thus, whatever the official verdict may be, it is doubtful if Mr Campbell has actually broken his previous record.

It is to be hoped that Mr Campbell can overcome his mechanical trouble and make two runs at the full speed of which his boat is capable, thereby removing this anomaly.

Yours faithfully,
R. G. HOWELLS,
Department of Physics,
University College of South Wales
and Monmouthshire,
Cathays Park, Cardiff.

Sir,—While agreeing in general with the conclusions arrived at by Mr R. G. Howells in his letter published to-day I must point out he is himself guilty of an all too common error. Immediately after the results of Mr Campbell's attempt at a record were published I sent a telegram to the official timekeeper pointing out that there appeared to have been a serious mistake in calculating the speeds, and received a reply stating that the speeds had been calculated in accordance with the international rules.

This was surprising to me as a similar mistake was made when Mr Campbell created his record in August of last year, and when I pointed this out I was given to understand that the Marine Motoring Association would take the necessary steps to get the international body to so revise their rules that they did not conflict with a simple mathematical law.

The fact is that it is impossible to average two varying speeds over a given distance by adding them together and dividing them by two. One might as well say that metres could be added to yards without first converting the one to the other. Speed in miles an hour is distance divided by time and one can average two varying speeds only by dividing the distance by half the total of the two times. Mr Howells is wrong in describing the figure of 225.63 as the arithmetic mean. It is an empirical figure of no significance, and the greater the difference in time between the two runs the farther from the true arithmetic mean is a result so arrived at.

I must also point out that although it is necessary to use decimals in making these calculations, to express the speed to hundredths of a mile an hour implies an accuracy in measuring both distance and time that is practically unattainable.

Yours faithfully,
T. W. LOUGHBOROUGH,
Secretary-General,
Fédération Internationale
Motocycliste,
The Old Forge, Hawkhurst, Kent,
Sept. 24.

BLUEBIRD'S SPEED

TO THE EDITOR OF THE TIMES

Sir,—However suspect the means of computing Mr Campbell's average speed, it remains true that it was computed by the same means as his previous record. Mr Howells is not therefore necessarily right in doubting if Mr Campbell has actually broken his previous record. But he could be.

Yours faithfully,
H. M. COLLINS,
West Kent Hotel, Bickley, Kent.

•••

Sir,—Mr Howells, while formulating an interesting arithmetical exercise in his letter of September 24, does less than justice to a very gallant effort. In every two-way timed trial, unless the times are identical, the mean of the separate speeds must be greater than the average speed over the combined distance, but the former basis of assessment is adopted by the responsible bodies as likely to reproduce more accurately the speed of which the car, boat or aircraft should be capable in neutral conditions of wind, gradient and other varying factors.

It is easy enough to imagine circumstances in which a closely contested record might be credited to one individual by the accepted method and to another by the 'average speed' method presumably advocated by Mr Howells, and it would be most unfortunate if the official ruling were liable to be called in question on some arithmetical juggle. Nobody can maintain that 225 m.p.h. represents Bluebird's maximum speed, but at least Mr Campbell has the consolation of knowing (subject to official confirmation) that his record is even less vulnerable to attack than before, and any contrary suggestion can only render disservice to the cause which he has striven so courageously and wholeheartedly to support.

Yours faithfully,
C. M. DAVIS,
22, Ember Lane, Esher, Surrey.

•••

Sir,—Of course Mr Howells is right. The 'average speed' of two runs of the same distance is the harmonic mean of the two separate speeds, and not the arithmetic mean as officially used. There is another very extraordinary thing, which Mr Howells does not mention. The timing is apparently done to the nearest tenth of a second, and with one run taking something of the order of 10 seconds only, the official and published speeds stating the second decimal places are quite unjustifiable. In fact both decimal places are meaningless, and even the units figure is suspect.

When the record was broken in 1955 I wrote to the Marine Motoring Association discussing the above two points. They informed me that they were putting the whole matter before their council, and that they would notify me of the result. Since then (August, 1955) I have heard nothing more from them.

Yours faithfully,
ARTHUR T. WALKER,
Senior Lecturer in Mathematics,
Woolwich Polytechnic,
9, Oatfield Road, Orpington, Kent.

•••

Sir, In Mr Howells's letter of September 24 he states: 'The arithmetic mean of the two speeds in opposite directions gives the true speed of the boat provided that the true speed is constant and that current and wind are also constant.'

Now this, I submit, is a fallacy, as is easily seen in the case of an aeroplane, where the effect of wind can be exactly determined. An aeroplane has, say a known constant speed of 100 m.p.h. It is to fly a test of 100 miles each way on an east-west course. In a dead calm it will take two hours. But if there is a 50 m.p.h. wind blowing, from west to east, the aeroplane will take two hours for the 100 miles east to west and 40 minutes on the return 100 miles west to east. So in this case it takes two hours 40 minutes for the 200 miles. That does not give the aeroplane's true speed.

The fallacy lies in the fact that the aeroplane is flying for a longer time in the adverse than in the favourable conditions. The only fair test would be by a timed run of, *e.g.*, one hour in each direction. Then on the east-west flight the aeroplane would do 50 miles in the hour and on the west-east 150 miles, i.e., 200 miles in two hours, so giving the true speed of the aeroplane. The arithmetic mean of the two speeds in opposite directions over a measured distance does not give the true speed when there is any wind or current.

Yours faithfully,
O. W. HARRIES,
Haselbury Plucknett, Crewkerne.

•••

BLUEBIRD'S SPEED

TO THE EDITOR OF THE TIMES

Sir, It is interesting to note the somewhat conflicting theories put forward by Mr R. G. Howells and others, which have appeared in letters published over the past week, concerning the method of calculating Mr Donald Campbell's recent world water speed record. Nevertheless, without going into lengthy technicalities I would make it clear both to them and to your readers that the advertised result of 225.63 statute m.p.h. was calculated by the official timekeeper and observed in strict compliance with the internationally recognized and accepted rules for such records, as laid down by the Union of International Motorboating, to whom all the details were submitted immediately following the attempt. Proof of accuracy is evident from their official confirmation of this record received by our association on October 1. This record has, of course, as correctly assumed by Mr H. M. Collins, been assessed by the same means as previous water speed records undertaken by Mr Campbell and others, both here and abroad.

While respecting the obvious capabilities of the various gentlemen in their own particular sphere who have advertised their theories, it must be appreciated that none of them has practical experience in the field of water speed records. Granted, theory is the basis of all serious application, but it can be proven only in practice. In so far as the computation of speeds for marine records is concerned, I would suggest that this is best left in the hands of men with years of practical experience of such matters being universally representative within the Union of International Motorboating.

Yours faithfully,
R. H. MEARS,
Assistant Secretary,
Marine Motoring Association,
148, Piccadilly, W1.

Sir,—In answer to Mr Howell's letter concerning the record set up by Mr Campbell in his boat Bluebird. I would like to point out that whichever mean is used, arithmetic, harmonic, or geometric, a different answer is obtained. It is for the parent body, which judges the authenticity of new world speed records, to decide which mean to use, in this case the arithmetic. As long as this conventional mean is recognized and used by all, the answer resulting from an attempt on the world speed record will bear a correct relationship to the existing record.

Yours faithfully,
M. A. BARTER,
20, Ryfold Road, SW19.

Sir,—No one can challenge the right of the Marine Motoring Association to perform any operations it likes on the data submitted to it, or to call the result by any name, real or fictitious. To that extent the protest of Mr Mears, whose letter you published on October 4, is justified. But the one simple inescapable fact remains that what was done in the case of Bluebird cannot possibly give the average speed of anything, whether on land or seat or in the air.

Yours faithfully,
C. W. PILKINGTON-ROGERS,
69, Welham Road, Retford,
Nottinghamshire.

Sir,—Mr Mears's letter of October 4 has the merit of originality. It is a novel proposition that a practical physicist and a mathematician are incompetent to comment on the calculation of speeds. Mr Mears is quite wrong in saying that any theories, conflicting or otherwise, have been put forward; as yet only simple mathematical and physical facts have been stated. No one has said that the speed of 225.63 m.p.h. was not calculated in accordance with the official rules; what has been said is that this figure has little connexion with reality.

Is it too much to expect that the Marine Motoring Association should give a reasoned reply to the points raised by Mr A. T. Walker and myself, and that they should not fob us off with a claim of the infallibility of the Union of International Motorboating?

Yours faithfully,
R. G. HOWELLS,
Department of Physics,
University College of South Wales and Monmouthshire,
Cathays Park, Cardiff.

BLUEBIRD'S SPEED

METHOD OF TIMING RECORD RUNS

Sir,—I have read the letters you have published on this subject with interest, and ever-increasing surprise at the amount of fallacy which can be packed into the discussion of such a simple subject.

I have so far refrained from challenging these fallacies, but in view of the astonishing statement made in Mr Pilkington-Roger's letter published on October 8 I feel the time has come when there should be some clarification as to the facts of this issue.

Nearly all the writer of the letters you have published seem to be obsessed with the idea that the object in calling for runs in opposite directions over the prescribed course is merely to double the length of the course, and that the result should be an average speed over the course so lengthened. This, of course, is a fundamental fallacy. The object in view is solely to eliminate, or reduce to an absolute minimum, the effects of extraneous variables which tend to obscure the true performance of the vehicle in question. The result is neither required nor intended to be the average speed over two lengths of the course, but the average speed over one length of the course. The arithmetic mean of the speeds achieved in the two directions gives in fact the true average speed over one length of the course.

Of the extraneous variables to which I have referred by far the most important is wind. Even in the simple case of human athletic endeavour a favourable wind can invalidate a record claim. In this case it is not feasible to eliminate the effect of wind by calling for runs in opposite directions, so the aspirant to a record must lose his claim if he has been favoured by a following wind of any appreciable strength.

In the measurement of air speeds, the effect of wind is of vital importance, because the aircraft is wholly airborne. In the case of records on land or water the effect of wind is very much smaller, and it is only because of this that certain controlling bodies have been able to get away with the fundamentally fallacious method of taking average times instead of average speeds.

From the very early day the Fédération Aéronautique Internationale recognized the vital importance of eliminating the primary factor of wind. This could only be done by calling for runs in opposite directions and taking the mean of the speeds achieved.

The fallacy of the mean time method can be seen at once by taking examples. Indeed, Mr O. W. Harries came very near in his recent letter to pointing out this fallacy by the example he took, but he failed to reach the logical conclusion. He took the case of an aeroplane with a known constant speed of 100 m.p.h. flying over a test course of 100 miles in each direction, with a wind of 50 m.p.h. blowing along the course. He pointed out that the aeroplane would take two hours in one direction and 40 minutes in the other. His argument then broke down because he added the times together and applied the total to a distance of 200 miles. As he rightly pointed out this did not give the aeroplane's true speed. What he should have done was to take the average of the two speeds, namely 50 m.p.h. in one direction and 150 m.p.h. in the other. This would have given him the correct result of 100 m.p.h. as the true speed of the aeroplane.

Mr Harries might have gone further and taken the case of a wind speed equal to the air speed of the aeroplane. In that case the speed in one direction would be nil and the speed in the other direction 200 m.ph., giving a correct mean speed of 100 m.p.h. On the basis of mean time, the time taken in one direction would be infinite, and the resulting speed of the aeroplane would work out as nil. The application of a principle to an extreme case so often gives a clear demonstration of its correctness or otherwise.

These examples illustrate the fact that the higher the wind speed the greater is the fundamental error of the mean time method. Even is the wind speed is only one-tenth of the true air speed of the aircraft, the error in the mean time method will be 1 per cent of the true air speed of the aircraft. This is four times as great as the permissible global error in the measurement and computation of speeds for records. Air speed record attempts are now usually made at very high altitudes, where winds up to 10 per cent of the fastest aircraft speeds are quite likely to be encountered.

So far as air speeds and records are concerned there can thus be no question of departing from the correct method, namely, to take the mean of the speeds achieved in opposite directions. As a precaution against the risk of appreciable variation in wind speed during the course of an attempt, the whole attempt must be completed within 30 minutes, and the two runs must be made at the same altitude within extremely close limits.

This same correct method was applied in the case of Bluebird. It is obvious from the speeds achieved in the the two directions that the second run was not done at full speed, but that does not affect the issue at all. By taking the average of the speeds in the two directions, the effect of wind was eliminated. To say that the resulting mean speed does not represent the maximum speed of which Bluebird is capable is quite irrelevant. A record does not set out to represent the maximum of which an aircraft, motor car, speedboat, or human athlete is capable; it simply represents what has been achieved on a particular occasion.

Yours faithfully,
R. H. MAYO,
Chairman, Records, Racing and Competitions Committee, Royal Aero Club;
Vice-President, Fédération
Aéronautique Internationale,
806, Beatty House, Dolphin Square,
SW1.

Source: *The Times*, September–October 1956

Extract from The Musgrave Ritual

"I was already firmly convinced, Watson, that there were not three separate mysteries here, but one only, and that if I could read the Musgrave Ritual aright I should hold in my hand the clue which would lead me to the truth concerning both the butler Brunton and the maid Howells. To that then I turned all my energies. Why should this servant be so anxious to master this old formula? Evidently because he saw something in it which had escaped all those generations of country squires, and from which he expected some personal advantage. What was it then, and how had it affected his fate?

"It was perfectly obvious to me, on reading the Ritual, that the measurements must refer to some spot to which the rest of the document alluded, and that if we could find that spot we should be in a fair way towards finding what the secret was which the old Musgraves had thought it necessary to embalm in so curious a fashion. There were two guides given us to start with, an oak and an elm. As to the oak there could be no question at all. Right in front of the house, upon the left-hand side of the drive, there stood a patriarch among oaks, one of the most magnificent trees that I have ever seen.

" 'That was there when your Ritual was drawn up,' said I as we drove past it.

" 'It was there at the Norman Conquest in all probability,' he answered. 'It has a girth of twenty-three feet.'

"Here was one of my fixed points secured.

" 'Have you any old elms?' I asked.

" 'There used to be a very old one over yonder, but it was struck by lightning ten years ago, and we cut down the stump.'

" 'You can see where it used to be?'

" 'Oh, yes.'

" 'There are no other elms?'

" 'No old ones, but plenty of beeches.'

" 'I should like to see where it grew.'

" We had driven up in a dog-cart, and my client led me away at once, without our entering the house, to the scar on the lawn where the elm had stood. It was nearly midway between the oak and the house. My investigation seemed to be progressing.

" 'I suppose it is impossible to find out how high the elm was?' I asked.

" 'I can give you it at once. It was sixty-four feet.'

" 'How do you come to know it?' I asked in surprise.

" 'When my old tutor used to give me an exercise in trigonometry, it always took the shape of measuring heights. When I was a lad I worked out every tree and building in the estate.'

" This was an unexpected piece of luck. My data were coming more quickly than I could have reasonably hoped.

" 'Tell me,' I asked, 'did your butler ever ask you such a question?'

"Reginald Musgrave looked at me in astonishment. 'Now that you call it to my mind,' he answered, 'Brunton *did* ask me about the height of the tree some months ago in connection with some little argument with the groom.'

"This was excellent news, Watson, for it showed me that I was on the right road. I looked up at the sun. It was low in the heavens, and I calculated that in less than an hour it would lie just above the topmost branches of the old oak. One condition mentioned in the Ritual would then be fulfilled. And the shadow of the elm must mean the farther end of the shadow, otherwise the trunk would have been chosen as the guide. I had, then, to find where the far end of the shadow would fall when the sun was just clear of the oak."

"That must have been difficult, Holmes, when the elm was no longer there."

"Well at least I know that if Brunton could do it, I could also. Besides, there was no real difficulty. I went with Musgrave to his study and whittled myself this peg, to which I tied this long string with a knot at each yard. Then I took two lengths of a fishing-rod, which came to just six feet, and I went back with my client to where the elm had been. The sun was just grazing the top of the oak. I fastened the rod on end, marked out the direction of the shadow, and measured it. It was nine feet in length.

"Of course the calculation now was a simple one. If a rod of six feet threw a shadow of nine, a tree of sixty-four feet would throw one of ninety-six, and the line of the one would of course be the line of the other. I measured out the distance, which brought me almost to the wall of the house, and I thrust a peg into the spot. You can imagine my exultation, Watson, when within two inches of my peg I saw a conical depression in the ground. I knew that it was the mark made by Brunton in his measurements, and that I was still upon his trail.

"From this starting-point I proceeded to step, having first taken the cardinal points by my pocket-compass. Ten steps with each foot took me along parallel with the wall of the house, and again I marked my spot with a peg. Then I carefully paced off five to the east and two to the south. It brought me to the very threshold of the old door. Two steps to the west meant now that I was to go two paces down the stone-flagged passage, and this was the place indicated by the Ritual."

Source: Sir Arthur Conan Doyle (1893) 'The Musgrave Ritual' in *The Memoirs of Sherlock Holmes*

Vermeer in perspective

'Knowledge becomes the painter,' Samuel van Hoogstraeten wrote in his 1678 *Inleyding tot de hooge schoole der schilderkonst* (Introduction to the School of Painting). Clio, muse of History, is depicted at the beginning of the chapter on the image and poetic inventions (*Poetisch verdichtselen*). More than ten years earlier, Vermeer had used Clio in his *Art of Painting* (Figure 10), in which he demonstrated not only his learning as a painter and inventor of allegories, but also, as we will see, his knowledge of perspectival theory.

In this large painting a heavy curtain appears to be held aside by an invisible hand: the viewer is invited to enter the painter's studio. The artist is seated, with his back toward us, and on his easel, on a grounded canvas, is an unfinished half-figure of Clio, sketched in white. The size of the canvas would not allow for a larger figure, nor for the trumpet of Fame, usually held by Clio.

The artist has started to paint at the top of the canvas. He seems to have finished the flesh colours and has begun to lay in the leaves of the laurel wreath. It looks like the painter—as pictured by Vermeer—is following tradition by finishing one area before setting up a new palette for the next area. . . .

A closed, bound book stands on end on the table in the middle ground of the *Art of Painting*, and an open book in folio appears at the right edge of the table, next to the painter's elbow. The inventory of Vermeer's estate, made in February 1676, lists a number of books in folio in a back room, and twenty-five other books of various kinds. It is conceivable that some of these were guides to perspective drawing, like the one by Hans Vredeman de Vries (1526/1527–1606) or the books published by Samuel Marolois (c. 1572–c. 1627), Hendrick Hondius (1573–1649), and François Desargues (1593–1662).

Vermeer was familiar with the principles of perspective described in these manuals, as can be seen in his paintings. Remarkably, thirteen paintings still contain physical evidence of Vermeer's system, by which he inserted a pin, with a string attached to it, into the grounded canvas at the vanishing point. With this string he could reach any area of his canvas to create correct orthogonals, the straight lines that meet in the central vanishing point (Figure 11). The vanishing point of the central perspective in the *Art of Painting* is still visible in the paint layer just under the end of the lower map-rod, below Clio's right hand.

To transfer the orthogonal line described by the string, Vermeer would have applied chalk to it. While holding it taut between the pin in the vanishing point and the fingers of one hand, his free hand would have drawn the string up a little and let it snap back onto the surface, leaving a line of chalk. This could then have been traced with a pencil or brush. Such a simple method of using a chalk line to make straight lines was probably used by Vermeer's Delft colleagues Leonard Bramer (1596–1674) and Carel Fabritius (1622–1654) to compose wall paintings, and is still used today by painters of *trompe l'oeil* interiors.

Figure 10 Johannes Vermeer canvas, Kunsthistorise

Little or no trace of Vermeer's method—except the pinhole—remains. This is visible to the naked eye on Vermeer's *Allegory of Faith.* Since almost all of Vermeer's grounds contain lead white, the loss of ground where the pin was inserted usually appears on the x-radiograph as a dark spot (Figure 12). This method of placing a pin through the canvas was not unique to Vermeer, but was in fact widely practiced among architecture painters of his time. It was used not only by Gerard Houckgeest (c. 1600–1661) and Emanuel de Witte (c. 1617–1692), but also by Vermeer's slightly older colleague Pieter de Hooch (1629–1684), a painter of interiors. Similarly, pictures by the genre painters Gerrit Dou (1613–1675), Gabriël Metsu (1629–1667), and others, also have irregularities in the paint surface where a pin was placed at the vanishing point.

Like most of his contemporary painters, Vermeer created the spatial illusion directly on the canvas. The Haarlem painter Pieter Saenredam (1597–1665) practised another method. On the basis of a preparatory sketch, observed first-hand, Saenredam constructed his perspective on a sheet of paper, later, in his studio. After having reached the final composition he would apply charcoal on the back of the paper and transfer the drawing with a sharp tool onto the surface of a prepared panel. After

this the painting process could start. Saenredam always used a panel support, while Vermeer apparently preferred to work on canvas.

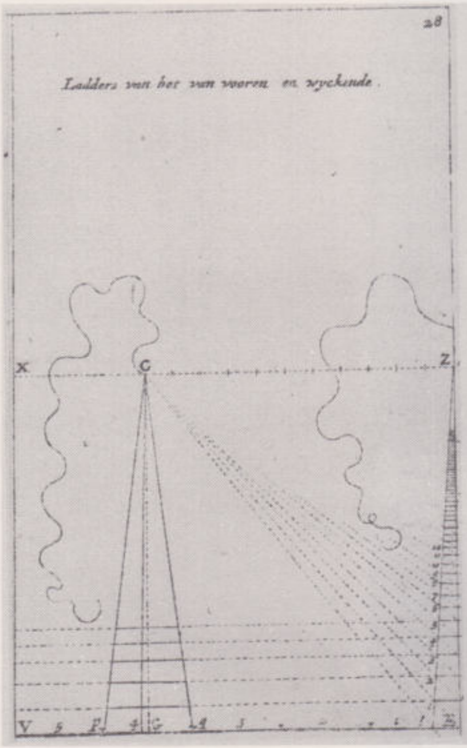


Figure 11 The construction of receding lines

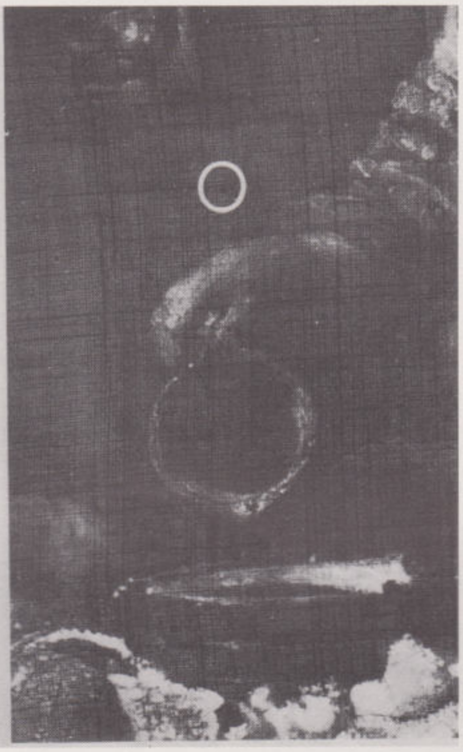


Figure 12 X-radiograph detail showing the pinprick by which Vermeer constructed the painting's perspective

In the beginning of his career Vermeer had difficulty in rendering floor tiles. The distance points, positioned at an equal distance on either side of the vanishing point on the horizon, provided the basis for the diagonals. These lines form the pattern of the floor tiles. When the horizon of his painting was relatively high and the distance points were close to the vanishing point, Vermeer apparently was vexed by the distortion of the tiles at the foreground corners. Examples of this occur in his earlier paintings such as *The Glass of Wine* and *The Girl with the Wineglass.* The last example in Vermeer's oeuvre that shows a certain distortion of the floor tiles owing to the short interval between the distance points is *The Music Lesson.* Here the view point, the centre of projection, is situated about 77 centimeters from the painted surface, the so-called picture plane. Viewed from this distance, the distortion is not noteworthy.

... It is interesting to note that Vermeer painted only diagonally placed floor tiles in his interiors, while De Hooch used diagonally placed as well as parallel tiles—sometimes even both within one painting—at random intervals.

Although Vermeer seems to have consistently used a string attached to a pin placed in the central vanishing point, the placement of the distance points poses a problem. At first one might expect that Vermeer determined the position of the diagonals on the edge of his canvas with the aid of a so-called 'height wall' (*hoogte muur*), as some Dutch landscape painters did. This would imply doing calculations or constructing of auxiliary lines in order to make space recede toward the back wall. Since

no trace of marks on the edges or elsewhere on his paintings has so far surfaced, it seems highly unlikely that Vermeer used such methods.

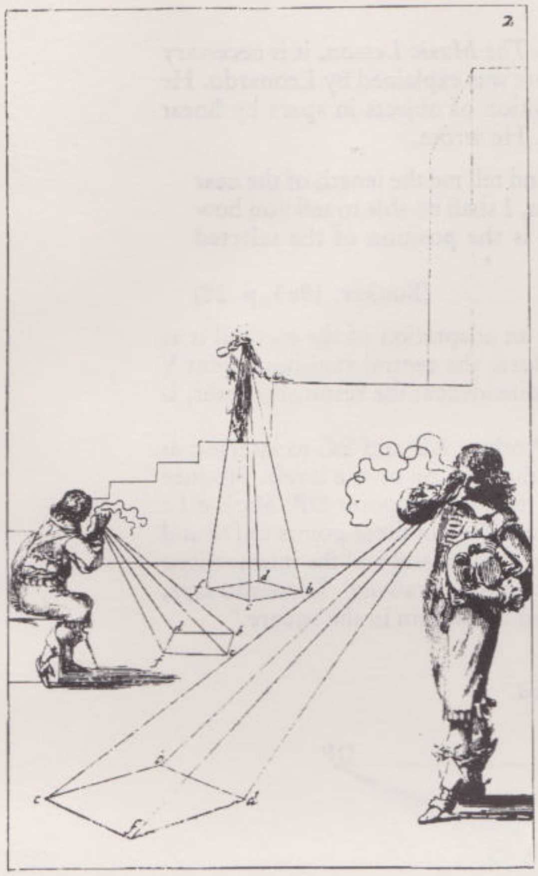


Figure 13

Painters would want to create perfect central perspective without having to struggle with complicated theories. One simple way was to use the already mentioned chalk line to determine the orthogonals, a method that Vermeer could apply to the diagonals as well. It can be assumed that Vermeer placed his canvas—usually small—against a board or a wall, with a nail on either side of the painting. These nails would be placed at the same level as the horizon in the picture. With strings attached to the nails Vermeer could again apply the chalk line for the diagonals in his constructions. The use of this simple method can be deduced from various manuals on perspective that Vermeer could have known. One such manual shows strings, held taut to one eye, attached to a square lying on the ground (Figure 13).

Source: Jorgen Wadum (1995) 'Vermeer in perspective' in *Johannes Vermeer* (catalogue of the Johannes Vermeer exhibition, 1995–1996, Washington and The Hague)

The photographic accuracy of Vermeer's paintings

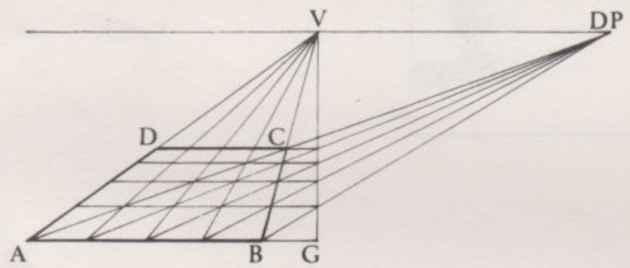
Before discussion can begin concerning Vermeer's *The Music Lesson*, it is necessary to understand a property of the 'distance point' that was explained by Leonardo. He showed how to obtain information about the position of objects in space by linear measurements taken from paintings and drawings. He wrote,

> If you draw a plan of a square (in perspective) and tell me the length of the near side, and if you mark within it a point at random, I shall be able to tell you how far is your sight from that square, and what is the position of the selected point.
>
> (Booker, 1963, p. 28)

Leonardo then goes on to explain the procedure; an adaptation of the method is as follows. (In Leonardo's description of the procedure, the central vanishing point V and the side of the picture plane VG are not in coincidence; the result, however, is precisely the same.)

Take a square ABCD in perspective (fig. 77). Produce AD and BC to intersect at V. The point V is the central vanishing point (on the horizon or eye level). Produce the diagonal AC to meet the eye level; this gives the distance point DP. Mark off a number of equal divisions along AG passing through B. Join these points to DP and V. The intersections on VG will give the horizontal coordinates and the intersections along AB will give the orthogonal coordinates. 'Scale your drawing', Leonardo says, 'and you will see the position of your point marked at random in the square.'

77. Leonardo's method.



In 1629, three years before Vermeer was born, an extensive work on perspective was published in Amsterdam by Samuel Marolois in which the principles that were laid down by Alberti were restated and elaborated. During Vermeer's working life in Holland, Descartes was inventing coordinate geometry and Spinoza was making the best lenses in Europe for telescopes and microscopes; from this time optical instruments were to become increasingly efficient.

This age of reason, when art and science were happily married, was characterized by measurement; measurement in terms of observation and measurement in terms of mathematics.

Vermeer, of all seventeenth-century artists, strove to achieve complete identity with

78. Johannes Vermeer: *The Music Lesson (A Lady at the Virginals with a Gentleman)*, c. 1665-70. Oil on canvas, 29 × 25.3in. [73.7 × 64cm.]. Royal Collection, The Queen's Gallery.

his subjects. It is therefore apposite to scrutinize and measure one of his pictures, *The Music Lesson* (*c.* 1665–70, 29in. × 25.5in. [73.7cm. × 64cm.] fig. 78). For linear measurements of a painting or photograph it is necessary to work perspective backwards, by Leonardo's method (fig. 77). (All measurements are quoted as they would be from the painting itself.)

1. Find the vanishing point at the intersection of the orthogonals.
2. Draw a horizontal line (eye level) through the vanishing point.
3. Extend the edges of the diagonally patterned squares on the floor to intersect the eye level.
4. Measure the distance from these intersections to the distance point: 27.6in. [70cm.] one side and 29.6in. [75.2cm.] the other side.
5. These distances should be equal; if they are not, take the mean, 28.6in. [72.6cm.]. This is the distance of the spectator (Vermeer) from the canvas. The discrepancies between the points where the squares converge on the eye level may be attributed to lens distortions or inaccurate drawing either by Vermeer or ourselves.
6. Measure the height of the lady in the painting, 10.2in. [25.9cm.].
7. Assume that her real height is 5ft., i.e. 60in. [152.4cm.]. In this case a guess based on the probability that Dutch gentlewomen in the seventeenth century were not generally very tall.
8. Let her real distance from the spectator be D inches.
9. Then by simiilar triangles (fig. 79):

$$\frac{D}{60} = \frac{28.6}{10.2} \quad D = 168\text{in.}$$

$$\left[\frac{D}{152.4} = \frac{72.6}{25.9} \quad D = 426.7\text{cm.}\right]$$

10. Measure the length of the diagonal of a square on the same level as the lady, 2.4in. [6.1cm.].
11. The real length of this diagonal must be

$$2.4 \times \frac{60}{10.2} = 14.1\text{in.}$$

$$\left[6.1 \times \frac{152.4}{25.9} = 35.9\text{cm.}\right]$$

12. By counting the squares on the floor in the painting, a ground plan of the room may be drawn up.
13. Draw in the picture plane and the spectator point on the plan.
14. Draw in the visual angle. This should meet the corner between the floor and

the left-hand wall at the point shown in the painting, providing an independent check of the system and the assumption made about the lady's height.

15. In this case the agreement is good; if it is only fair then the process from (6) must be repeated, making a new assumption: this process must be continued until the necessary accuracy is obtained.

16. To find the dimension of any other object in the painting, compare its measurements with those of a square at the same distance in space from the spectator. If the picture length of the diagonal of the square is **L**, and the picture measurement of the object is **m**, and the true measurement of the object is **M**, then:

$$\frac{M}{m} = \frac{14.1\text{in. [35.9cm.]}}{L}$$

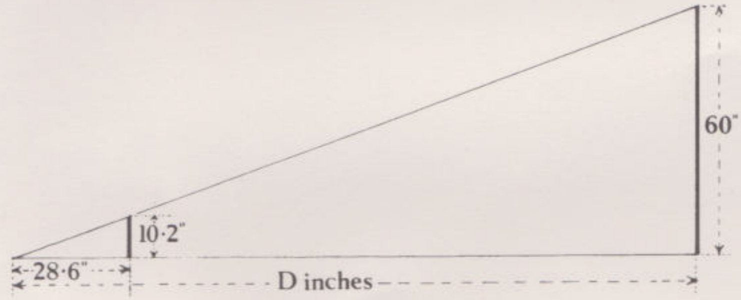
$$M = 14.1\text{in. [35.9cm.]} \times \frac{m}{L}$$

17. By this means the size of any object in the painting may be found and the plan and elevation views drawn, providing all the necessary information for an axonometric drawing (fig. 80) or even a *tableau vivant.*

As we shall see (fig. 87), when the visual angle exceeds about 25° distortions occur towards the edges of the pictures. In *The Music Lesson*, where the visual angle is 45°, the squares at the bottom of the picture begin to distort. Vermeer disguised this on the right-hand side of the picture by covering the squares with a carpet, and on the left-hand side by cutting the squares with the picture frame. The disadvantages of possible distortions were probably carefully weighed against the feeling of intimacy that he achieved by drawing the spectator into the room, to sit by the table and observe the music lesson. The plan also reveals the care with which Vermeer placed the objects on the room. Note how the harmony of movement in space is obtained by placing the figures and the furniture either parallel to the picture plane or following the diagonal pattern of the squares on the floor.

The calculations may be checked optically by using a similar tracing apparatus to

79. Similar triangles: the height and distance of the lady in the painting compared with the height and distance of the real lady in the scene.



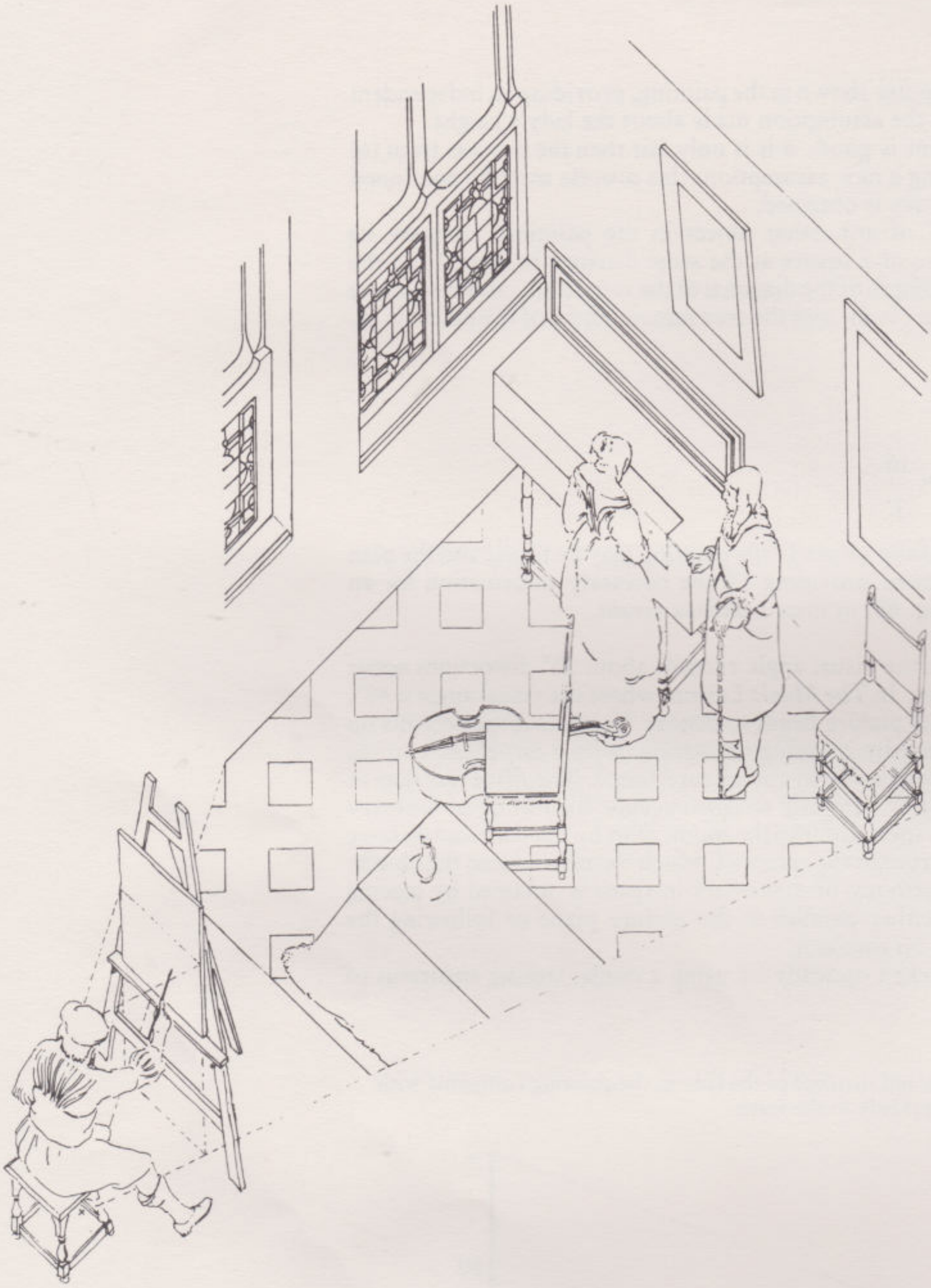


fig. 70, with a fixed eye piece at 28.6in. [72.6cm.] from the picture plane, and employing the services of a 60in. [152.4cm.] lady standing 168in. [426.7cm.] from the eye piece. It will be seen that Vermeer painted *The Music Lesson* sight size; and the image of the lady on the screen will be 10.2in. [25.9cm.] which is the same as in the real painting.

The photographic accuracy of Vermeer's paintings suggests that he may have used a camera obscura; there are, however, practical difficulties involved in using one in poor lighting conditions indoors. Even now, when large lenses are relatively cheap, the subject requires considerable illumination for an image to be clearly visible; the softly modulated lighting that is found in Vermeer's pictures could not have been observed in a camera obscura. Another possibility is that Vermeer (and perhaps Rembrandt and Velazquez) made use of mirrors[2]. This would have helped them to obtain an accurate drawing on the mirror, to reduce the scale of the subject (very necessary, as studios were often quite small) and to close up the tone values (silver, not mercury, was used as a reflector in the seventeenth century, causing a 30 per cent reduction of the light received).

If Vermeer used a mirror for *The Music Lesson* he would have been seated close to the table in the foreground of the picture with his back to the scene; in addition he would have had to exclude himself from the picture. The essential geometry that has been described in the preceding account would, however, remain the same.

80. Axonometric projection derived from *The Music Lesson*.

Source: Fred Dubery and John Willats (1983) *Perspectives and Other Drawing Systems*

Acknowledgements

Grateful acknowledgement is made to the following sources for permission to reproduce material in this booklet.

Jung, C. G. (1963) *Memories, Dreams, Reflections*, HarperCollins/Routledge and Kegan Paul.

Windsor, J. (1994) 'What price yesterday's pound in our pocket?', *The Independent*, 26.2.1994 © Independent Newspapers.

Low Pay Unit (1993) 'No let-up in growth of low pay', *The New Review*, December 93/January 1994.

Cohen, B. I. (1984) 'Florence Nightingale', *Scientific American*, March 1994 © Scientific American Inc. Diagrams: p. 14: Thomas M. Pantages, courtesy of Houghton Library, Harvard University; pp. 16, 17, 19: Thomas M. Pantages, courtesy of Widener Library, Harvard University. Photograph: p. 15: courtesy of the National Portrait Gallery, London.

Paulos, J. A. (1990) 'Chance encounters' in *Innumeracy: mathematical illiteracy and its consequences*, Penguin Books © 1988 John Allen Paulos.

Koestler, A. (1974) *The Roots of Coincidence*, Picador.

Harris, M. P. (1993) 'Guillemot' in Gibbons, D. W., Reid, J. B. and Chapman, R. A. (eds) *The New Atlas of Breeding Birds in Britain and Ireland: 1988–1991*, T & AD Poyser © British Trust for Ornithology, Scottish Ornithologists' Club, Irish Wildbird Conservancy.

Wright, D. (1995) 'Blindly into the ditch', *Times Educational Supplement*, 31.3.1995 © David Wright.

Gould, P. (1994) 'Mapping the AIDS pandemic', *Geographical*, Vol. LXVI, No. 1 © Centaurian Publications Ltd.

Wood, D. (1994) 'Images of the world', *Geographical*, Vol. LXVI, No. 12 © Centaurian Publications Ltd.

Tufte, E. R. (1983) *The Visual Display of Quantitative Information*, Graphics Press. Diagram from Marey, E. J. (1885) *La Méthode Graphique* by permission of the British Library.

McClain, E. G. (1978) *The Pythagorean Plato* © 1978 Nicolas Hays Ltd.

Kitson, T. (1984) 'The ultimate mile', *New Scientist*, 2.8.1994 © IPC Magazines Ltd.

Whipp, B. J. and Ward, S. A. (1992) 'Will women soon outrun men?', *Nature*, **355**, 2.1.1992 © Macmillan Magazines Ltd.

Steen, L. A. *et al.* (1991) 'Sorry, no King Kongs' in *For All Practical Purposes: introduction to contemporary mathematics* (second edition) © 1988, 1991 COMAP Inc., W. H. Freeman and Company.

Extracts from OTIS technical documents © 1993 OTIS plc.

Howells, R. G., Loughborough, T. W., Collins, H. M., Davis, C. M., Walker, A. T., Harries, O. W., Mears, R. H., Barter, M. A., Pilkington-Rogers, C. W. and Mayo, R. H. (1956) Letters to *The Times*, 19.9.56–9.10.56.

Conan Doyle, A. (1893) 'The Musgrave Ritual' in *The Memoirs of Sherlock Holmes*, © Northolme Limited; reproduced by kind permission of Jonathan Clowes Ltd, London, on behalf of Northolme Limited.

Wadum, J. (1995) 'Vermeer in perspective' in Wheelock, A. K. jr (ed.) *Johannes Vermeer*, © 1995 Board of Trustees, National Gallery of Art, Washington, and Royal Cabinet of Paintings Mauritshuis, The Hague; reproduced by permission of Yale University Press. Figure 10: Jan Vermeer, *Painter in his Studio*, Kunsthistorisches Museum, Vienna. Figures 11 and 13: Abraham Bosse (1684) *De Figuren en Redeneeringe tot de Praktyck der Perspective*, Koninklijke Bibliotheek, The Hague. Figure 12: Jan Vermeer, X-ray detail of *A Maidservant Pouring Milk*, Rijksmusuem, Amsterdam.

Dubery, F. and Willats, J. (1983) *Perspectives and Other Drawing Systems*, The Herbert Press © Fred Dubery and Dr John Willats. Photograph: Jan Vermeer, *Lady at the Virginals with a Gentleman* or *The Music Lesson*, The Royal Collection © Her Majesty the Queen.

Every effort has been made to trace all copyright holders, but, if any has been inadvertently overlooked, the publisher will be pleased to make the necessary arrangements at the first opportunity.

Printed in the United Kingdom by Hobbs the Printers Ltd,
Brunel Road, Totton, Hampshire, SO40 3WX.

SUP 34314 2
1.4